

HEAD'S CONCEPT OF THE SCHEMA AND ITS APPLICATION IN CONTEMPORARY BRITISH PSYCHOLOGY

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PART I. HEAD'S CONCEPT OF THE SCHEMA

- I. *Introduction* (pp. 267-268).
- II. *The appreciation of posture and passive movement* (pp. 269-273)
 - (1) *Munk's theory* (pp. 269-271).
 - (2) *Head's criticism of Munk* (p. 271).
 - (3) *Head's concept of the schema* (p. 272).
 - (4) *Examination of Head's evidence* (pp. 272-273).
- III. *Head's theory of the schema* (pp. 273-285).
 - (1) *Posture* (pp. 273-277).
 - (2) *The maintenance of static tone* (pp. 277-278).
 - (3) *Projection* (pp. 278-283).
 - (4) *Vigilance* (pp. 284-285).
- References* (p. 285-286).

I. INTRODUCTION

Within my corner I will take my place.
And grant me grace
Some delicate thing to perfect and complete
With passionate contentment as of old
Before my heart grows cold.

HENRY HEAD

IN the present state of psychological science the value of theoretical formulation and interpretation must be evaluated rather in terms of its fruitfulness in promoting experiment than by reference to its internal consistency. The time has yet to come, we believe, when large-scale conceptual systems can be erected with any likelihood that they will be found to correspond in detail with equally large bodies of fact. Accordingly, in endeavouring to trace the development and applications of the concept of *schema* we shall make criticism subordinate to exposition. Our object will be to provide a connected account of the sequence of

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ideas which, in the hands of Bartlett especially, has led to the provisional establishment of what we believe to constitute a truly novel approach to some of the fundamental problems of psychology. Where comment or criticism will serve to make clearer the ideas we desire to expound, we shall try to provide them. But where criticism is not forthcoming, it must not be assumed that we regard the views in question as being in any sense beyond its reach. Purely destructive analysis would, indeed, be as easy as it would be unprofitable. Constructive criticism, and the extension of the theory beyond its present published limits, we leave to the future.

Historically, the theory must be regarded as in some sense a justifiable retreat of explanatory pretensions before a fuller apprehension of the complexity of presented fact. It arises essentially in the failure of nineteenth-century systems of interpretation. These, endeavouring to construct the basic mechanisms of perception, memory, thought and language out of elements which were the product of abstraction rather than experience, were eventually brought to a standstill by the failure of the picture they painted to resemble its original. This breakdown brought about a more thorough examination of the facts demanding explanation, and greater caution in the use of conceptual schemes of interpretation. In the study of aphasia, memory and thought, to name but three instances, there arose a tendency to introduce new concepts, not for the purpose of erecting fresh scaffolds of deductive theory, but in order to permit the accurate characterization of phenomena within the limits imposed by available methods of investigation. The recognition that the description of fact in a manner suited to further progress is dependent upon the existence of an adequate conceptual basis is implicit in the work both of Head and of Bartlett. Implicit, too, is the hope that out of this somewhat fluid conceptual system there will eventually crystallize the means of attempting once again the construction of large bodies of explanation internally integrated by relations of a deductive character. But this step must wait upon the further development both of factual knowledge and of conceptual and linguistic invention. For the present the views we shall expound must be judged as a strategical retreat rather than a brilliant sally.

II. THE APPRECIATION OF POSTURE AND PASSIVE MOVEMENT

(1) *Munk's theory*

Head's notion of the *schema*¹ was introduced in the first instance in order to account for the appreciation of passive movement and the recognition of postural change. He maintained that the impairment of these functions found in certain cases of cortical injury could not be adequately interpreted in terms of the existing doctrines of cortical function, and singled out for detailed criticism the theory of Munk as representative of contemporary explanation.

On the basis of his extensive extirpation experiments, Munk⁽⁶⁾ had advanced the view that the so-called *Fühlsphäre*² is directly concerned with the appreciation of touch, pressure, temperature and muscle sensations. With the exception of those of the last group, these sensations could be reactivated centrally in the form of images. The muscle sensations, though not themselves capable of imaginal revival, could nevertheless combine with the contact or pressure sensations to give rise to contact or pressure images. "And, moreover, the two kinds of sensations combined go to provide the definite and distinct images of the actual and momentary positions of the different parts of the body, and of the alterations in position of the various parts of the body in the case of passive movement" ((6), p. 32).³

On this view, the mechanism of active movement involves two sets of factors. The first comprises what Munk called sensations of innervation, and the second, images of movement. Sensations of innervation were defined as the perceptions of active movements of the various parts

¹ The choice of the term *schema* is, as Bartlett has pointed out, in many respects an unfortunate one in virtue of its many and varied connotations. In particular it is essential to point out that the term has nothing in common with Kant's use of the same word. On the other hand, it is perhaps worth noting that, in their functional though not in their generic aspects, there are certain resemblances between the *schemata* of Head, and the *pure intuitions* of Space and Time, as expounded by Kant in the *Aesthetic*. It is one of the functions of the Head *schema* to measure up and articulate the crude impressions of sense before they can be apprehended consciously. The Kantian pure intuitions have much the same function in articulating sense-impressions and rendering perception possible. Another example of the use of the term *schema* is to be found in the writings of Selz⁽¹³⁾. There are certain obvious affinities between his use and that of Bartlett, but these will not be further discussed in this paper.

² Munk used the expression *Fühlsphäre* to denote the part of the cortex concerned with the initiation and elaboration of motor activity generally. In view of Munk's somewhat peculiar treatment of cortical function, it has been thought better to preserve his original term.

³ We alone are responsible for the translations from the German of Munk.

of the body. Munk's theory of their origin is somewhat curious. He supposed activity on the part of those spinal and midbrain centres concerned with the initiation of movement to be represented in consciousness as sensations of innervation. These, however, he believed always to arise in conjunction with the pressure and muscle sensations, and to become integrated with them in the elaboration of images of movement. "For each co-ordinated and adapted movement", he writes, "the three types of sensation stand in a specific and definite relationship to one another, and combined, they permit firstly the arousal of images of movement—the images of the active movements of the parts of the body—and secondly the arousal of the touch images—the images of the form, extension, etc.—of the objects which are touched by the moving members of the body" ((6), p. 33).

The sensations of innervation were considered by Munk to be of paramount importance in the elaboration of images of movement. These images he supposed to be developed in infancy out of purely reflex movements; and, in later life, the images corresponding to movements of a given part of the body could arise in the *Fühlsphäre* despite the fact that this organ is not normally concerned directly with the initiation of the movements in question. Moreover, after extirpation of a specific region of the *Fühlsphäre*, Munk believed that the lost images of movement could be built up anew out of the reflex and locomotory movements of the body.

Munk claimed that the results of his extirpation experiments not only established his general theory, but also led to the conclusion that there is strict localization of these various *perceptual* functions in the *Fühlsphäre*. "The parietal lobe cortex of the dog is the *Fühlsphäre* for the opposite side of the body. It can be divided into a number of areas, with each of which is correlated some specific part of the corresponding side of the body. The fibres conveying the skin, muscle and innervation sensations from the side of the body in question terminate in the central, perceiving, elements of each such area, and within these areas, the images of the corresponding parts of the body have their seat. Each specific area thus constitutes the independent motor cortex (*Fühlsphäre*) of the corresponding part of the body" ((6), p. 38).

The effect of lesions in various parts of the motor cortex was explained by Munk in the following manner: "Small lesions in the various cortical areas result in partial loss of images of the corresponding part of the body; larger lesions the complete loss of the images in question. Psychic disablement (*Seelenlähmung*)—psychic paralysis and psychic

anaesthesia—of the corresponding part of the body is the outcome. The lost images can nevertheless build themselves up anew in the remainder of the *Fühlsphäre*. The sensations are themselves affected in the event of more extensive extirpations, and in such cases the images can be reinstated only in part. The disturbance and incompleteness of restitution is greater the smaller the amount of cortex remaining intact. Total destruction of the *Fühlsphäre* corresponding to a given part of the body must inevitably result in the permanent abolition of all sensation and images appertaining to that part of the body. This is 'cortical disablement'—(*Rindenlähmung*)—cortical paralysis and cortical anaesthesia" ((6), pp. 38-9).

(2) *Head's criticism of Munk*

Munk's conception of the motor cortex as a repository of images of movement was sharply contested by Head⁽¹⁾, who maintains that the term 'image' strictly refers only to that which can be voluntarily recalled into consciousness. He points out that if we sit immobile, and imagine our fingers touching some object, the only image in consciousness is a visual one. Head then proceeds to adduce clinical evidence from cases of cortical lesion to the effect that appreciation of posture and of passive movement may be impaired or abolished without any loss of power to visualize the position of the affected part of the body. "Place the patient's affected arm in front of him on the bed, allowing him to see the position in which it lies; close his eyes, and in most cases he will see a mental picture of his hand. Then change its position while his eyes remain closed and he will continue to see a picture of the hand in its old position. Moreover, if localization is not affected, he will name correctly the spot stimulated but will refer it to the position in which he visualizes the hand. The visual image of the limb remains intact, although the power of appreciating changes in position is abolished" ((1), p. 605).

Head, whilst acknowledging that Munk was almost alone in perceiving the fact that some standard deriving from past experience is necessary to the appreciation of posture and of passive movement, argues that this standard cannot be a visual one. He likewise maintains that "images of movement", by which he appears to mean kinaesthetic imagery, cannot fulfil this function of supplying postural standards. It must, however, be pointed out that no experimental data are quoted in support of this last contention.

(3) *Head's concept of the schema*

In view of the considerations summarized above, Head attempts an alternative hypothesis: "...the image", he writes, "whether it be visual or motor, is not the fundamental standard against which all postural changes are measured. Every recognizable change enters into consciousness already charged with its relation to something that has gone before, just as on a taximeter the distance is presented to us already transformed into shillings and pence. So the final product of the test for the appreciation of posture or passive movement rises into consciousness as a measured postural change.

"For this combined standard, against which all subsequent changes of posture are measured before they enter consciousness, we propose the word 'schema'. By means of perpetual alterations in position we are always building up a postural model of ourselves which constantly changes. Every new posture or movement is recorded on this plastic schema, and the activity of the cortex brings every fresh group of sensations evoked by altered posture into relation with it. Immediate postural recognition follows as soon as the relation is complete" ((1), pp. 605-6).

(4) *Examination of Head's evidence*

Can we assess the extent to which Head's 'refutation' of Munk's doctrine is justified? In the first place we may reasonably ask whether the results of Head's observations upon the relation of imagery to postural recognition inevitably necessitate the abandonment of Munk's position. Head's experimental data, it must be emphasized, are relevant only to the question whether visual imagery plays any significant role in the appreciation of postural change. It will be remembered that Munk makes no explicit mention of visual imagery in this connexion, yet when he speaks of images of movement it is clear that he has some purely psychical product in mind. The upshot of Head's criticism is really that postural recognition in normal subjects habitually and demonstrably takes place without the intervention of introspectively detectable images of any kind whatsoever.¹ One may perhaps conclude that although

¹ Prof. Bartlett, in discussing this paper with us, has called attention to a rather different interpretation of Head's position. He suggests that the presence or absence of images in consciousness is a relatively unimportant factor in the argument. What is important, however, is the presumption that images *cannot* furnish a standard of the kind required for appreciating position. An image, Bartlett argues, is always static and always constitutes a definite event in the continuity of mental life. Whatever items may be supposed to underlie postural recognition must, on the other hand, *literally disappear and reappear only in the result*—namely in direct awareness of position. The image, by its very nature, cannot furnish the necessary basis for assessing postural change.

Head has failed to produce an unequivocal refutation of the earlier theory, he has emphasized forcibly the highly improbable character of Munk's hypothesis.

III. HEAD'S THEORY OF THE SCHEMA

(1) *Posture*

The mechanisms of postural co-ordination must evidently involve activity at a number of levels in the nervous system. Among the many problems associated with these mechanisms, Head was pre-eminently concerned with the nature of those afferent co-ordinations which must directly underlie voluntary adjustive activity. In the first place, he points out, postural recognition is not constantly in the central field of attention, but always forms the measure against which we judge subsequent postural change. In the second place, he denies the possibility of a direct perception of position, and maintains that the appreciation of posture and passive movement must necessarily presuppose some background of comparison and relation. In postural recognition, awareness of an altered position is immediate, yet the postural sensations rise into consciousness "charged with the relation to something that has happened before". According to Head, immediacy of presentation on the one hand, and relation to the past on the other, can be adequately reconciled only through postulation of the sensory *schema*. We must therefore examine more closely the character of the *schema*, and the part Head believes it to play in the maintenance and regulation of posture.

The schema may perhaps best be envisaged as a 'plastic model' of the individual's own body in space and time—a kind of *homunculus* upon which movements are successively registered as they occur. A continually self-compensating basis of comparison is thus provided.

An analogy may perhaps best serve to make the essential nature of this view clearer. Let us suppose that we are driving a motor car upon a desert without any effective landmarks, and are equipped only with a milometer and a compass. We require to keep track of our position, but it is not practicable to drive along a straight course from our starting point. In the first place it is evident that, in the midst of the desert, 'position' has no effective significance except in relation to some fixed point through which we have previously travelled. And this is analogous to Munk's assertion that the recognition of posture cannot take place independently of the adoption of some standard deriving from the past. But the question analogous to that raised by Head is that of how it is possible for us at any given moment to determine our position. It is

plain that merely reading the compass to determine the direction of travel, and the milometer to determine the distance travelled from a fixed point already passed through will not suffice. It is essential that both distance travelled and direction shall have been *continuously recorded* throughout the entire journey from the fixed point. This corresponds to Head's assertion that no single element from the past will suffice as the basis of recognition of posture. The standard must be a continuously variable one, involving in an important sense all that has gone before. But, in addition, if this continuous record has in fact been kept (and it is easy to imagine some mechanism by which compass and milometer are made jointly to move a point recording on paper), then there is no need for any calculation in order to determine our position, since it is directly indicated upon the recording apparatus. This is analogous to Head's statement that there is direct perception of posture unmediated by any preliminary conscious activity. It is of the very nature of the schematic mechanism, which allows any recognition of posture to be made at all, that it affords that recognition directly. It is in some such way as this that we must envisage the concept of schema.

Head makes it plain that schemata are to be envisaged as *physiological* dispositions.¹ "...the activities on which depend the existence and normal character of the schemata lie for ever outside consciousness; they are physiological processes with no direct psychical equivalent" (1), p. 723).

The part played by the schema in the maintenance and regulation of posture may perhaps be summarized in something like the following terms: In the first place, incoming impulses from the periphery are continuously recorded on, or by, the schema; a tally is thus kept of the state of the organism at every moment which affords the necessary basis for the proper use of those impulses that follow. In the second place, the activity of the cortex brings every fresh group of sensory impulses evoked by altered posture into relation with the continuously altering record provided by the schema. Postural changes may, or may not, invoke changes in the central field of consciousness. In either case the initiation of change demands the utilization, on the physiological level, of the product of reaction between incoming impulses and schema. If

¹ Head refers to postural schemata sometimes as *dependent upon* physiological dispositions, sometimes as the dispositions themselves. In saying that the schema is *dependent upon* the physiological disposition, we believe that Head was emphasizing a purely formal aspect of the operation of these dispositions, and did not intend to postulate a distinct though dependent entity. Thus one may distinguish the shape of an organism from the material constituting it despite the obvious dependence of the former upon the latter.

the change does make itself felt in the central field of consciousness, then such conscious change is the direct product of an already completed reaction between present and past carried out by the activity of the cortex at the physiological level. The process whereby incoming impulses and schemata react, whilst purely physiological, involves both comparison and relation, and may be regarded as *analogous* to a conscious act of comparing and relating. Finally, in the third place, the incoming impulses themselves modify the schema, leaving their own record in a form already related to the past, and so putting the mechanism in a state of readiness to deal with the next incoming group.

The existence and proper operation of the schemata depend upon the functional integrity of the cortex. Cortical lesions, by impairing or destroying the schemata, concomitantly abolish the recognition of posture on the affected side of the body. "Not uncommonly a patient with a cortical lesion can recognize that some movement has occurred, but is entirely unable to discover its direction or amplitude; complete appreciation of passive movement is the recognition of serial changes in a certain direction" (1), p. 604).

One example of defective appreciation of posture has already been given (vide p. 271 *supra*). In another case quoted by Head, a leg was lost some time before the appearance of the lesion which destroyed the power of recognizing posture. Sensations of movement in the phantom leg ceased on the occurrence of the cerebral lesion—"the stroke which abolished all recognition of posture destroyed at the same time the phantom limb" (1), p. 606).

While the schemata are thus subject to impairment or abolition in consequence of certain injuries to the cortex, their existence also demands a *continuous* supply of incoming impulses from the part of the body concerned. A patient whose little finger was completely deprived of sensibility, owing to section of the ulnar nerve by gunshot, recognized the existence of this finger as a dead part of himself. After five months, however, it became necessary to amputate through the lower third of the arm. The phantom hand which appeared subsequently had only four digits. "During the five months of total insensibility, the schemata associated with the little finger, no longer reinforced from the periphery, had gradually died away..." (2), pp. 489-90).

Beyond thus indicating the general physiological conditions upon which the existence and functioning of the schemata depend, and introducing in this connexion the concept of 'vigilance' (of which a brief account will be given later), Head does not attempt any closer enquiry

into their anatomical and physiological nature. On the other hand, the destruction of schemata does not exhaust the functional disturbances entailed by cortical injury. Manifesting itself in combination with the consequences of schematic destruction, but essentially independent of them, is the loss or impairment of what Head calls *local attention*. If psycho-physical judgements regarding sensations from the affected part of the body are required, the patient's responses are uncertain and variable. Hallucinatory sensations are apt to appear in the absence of an appropriate stimulus. Sensations initiated by a stimulus may persist beyond its duration, and the intensity of a sensation is not graded according to the strength of the stimulus. In fact the patient's behaviour, Head considers, presents an exaggerated picture of the normal individual giving psycho-physical judgements while in an inattentive state of mind. On this account, he suggests that the defect consists in a loss of 'local attention'. "When the affected part is under examination, the patient's replies are uncertain and variable, . . . and yet, as soon as we examine any unaffected part, the patient may come through the same series of tests without a mistake¹. . . but the patient's power of general attention has not been lowered. . . . Records from the limbs of the sound side show that the phenomena of defective attention are confined to the abnormal part. A portion of the patient only has, as it were, become untrustworthy" (1, pp. 606-7).

In summing up, Head concludes that "...the cerebral cortex is the organ by which we are able to focus attention upon the changes evoked by sensory impulses. . . . Uncertainty of response destroys all power of comparing one set of impressions with another and so prevents discrimination" (1, p. 607). It would appear, as we have already noted, that Head regards this consequence of cortical lesions as distinct from those connected with the destruction of schemata. In particular, according to his analysis, it concerns the level of consciousness. But we believe that it would be possible reasonably to interpret what appears behaviourally as loss of local attention in terms of the failure of the process of reaction between schemata and incoming impulses. Thus the sensations concerned in the psychophysical judgement would arise in consciousness lacking in that essential determination in respect to what has gone before, which alone can make them the effective conditions of an accurate judgement. It is possible, indeed, that some such view was in Head's mind, but, if this be the case, his use of the notion of local

¹ That is, without making mistakes in relation to the relative intensities of supraliminal stimuli.

attention, even as a figure of speech, is definitely misleading. In any case, it will be unnecessary to consider further this type of defect.

(2) *The maintenance of static tone*

The presentation to consciousness of data relating to the position of the body in space, and the indirect regulation of its movement, do not constitute the only functions of the postural schemata. They are also held responsible for the maintenance and regulation of tonic innervation. "The only constant and continuous record of our bodies in space", writes Head, "exists in the condition of our schemata. These physiological dispositions of the cortex ensure that the inception of a voluntary movement will find the part to be moved in a suitable attitude. For, unless postural impulses perpetually modified these unconscious physiological activities consonantly with every change in position, we might will a certain movement although the limb was not in a suitable attitude to bring it into being... But in order that the part of the body to be moved may be ready to spring off, like a runner, at the word of command, its static tone must be normal. Should it be poised atonically, the voluntary motor act has first to gather in slack before the limb begins to move. Normal posture and normal tone are coincident terms" (1), p. 724).

Head considers the maintenance and co-ordination of static tone to be the product of a "constant stream of influence" exerted by "the simple existence of the physiological dispositions which we have called schemata". He supposes this automatic influence to be exerted directly upon the highest efferent centres without the intermediation of consciousness, and shows that it may be grossly disturbed as a consequence of cortical lesions. "It is obvious", Head observes, "that any lesion which disturbs postural schemata will interfere with static tone. For in order that a part of the body at rest may retain a normal posture, afferent postural impulses must exert a constant influence on the activity of the appropriate receptive centres of the cortex. These are the repository of spacial¹ schemata. The physiological changes brought about by this stream of afferent impressions not only checks and controls (*sic*) voluntary movements, but ensures that the static tone of the part shall

¹ Head frequently uses the expressions "spacial schemata" and "postural schemata" interchangeably. It would appear to us that by the expression "spacial schemata" Head meant to include not only the postural schemata already described, but also the schemata concerned with projectional surface localization. These will be briefly dealt with in the next section.

be adapted to maintain its position. Consciousness is in no way necessary for such co-ordination; in fact the regulation of tonic innervation occurs entirely on the physiological level. Any lesion, which tends to destroy postural schemata, not only disorders voluntary movement, but under suitable conditions may diminish static tone" (1), p. 724).

The hypotonia so closely associated with defective recognition of posture and passive movement in the absence of any defect in the highest motor centres, Head treats as a false co-ordination. In the first place, he points out, the receptive centres in the cortex are "badly informed" of the position of the affected parts. (By this, he presumably implies some direct interference with afferent pathways.) In the second place, the schemata which exist in these receptive centres are themselves disordered. Such disturbance results in defective co-ordination of voluntary movement and concomitantly affects the static tone of the parts concerned. The effect is contrasted by Head with the disturbance which may result from a subcortical lesion, where the permanent dispositions of the afferent centres remain intact, and gross impairment of static tone may be absent, unless it be the result of pathway interference (1), pp. 724-6).

(3) *Projection*

By projection Head means, broadly, the reference of a sensation to an object. His theory of the projectional aspects of sensation and of the schemata which underlie them is intimately bound up with his general theory of sensation. The latter involved the distinction of protopathic and epicritic types, and made much use of the Jacksonian concept of levels.¹ Further, Head insisted throughout upon the complexity of the integrative processes and the competition of incoming impulses at the highest physiological levels. All these diverse aspects of the general theory must be borne in mind in reading the discussion that follows.

As we have said, broadly speaking projection is the reference of a sensation to an object. But as reference is complex, an injury or disease

¹ Head's intellectual debt to Jackson is so many-sided that it becomes difficult to assess in the treatment of any one problem. But it is certain that the concept of schemata was foreshadowed in Jackson's general theory of third-degree cortical representation and it probably owes much to his notion of 'constants' (cf. Jackson(3), especially pp. 41-2, 54, 96, 98-118). While we have not attempted to trace the evolution of Head's thinking from Jackson's views, it is plain that the concept of schema is not only consistent with, but actually presupposes, the Jacksonian theory of cortical representation. We may also note that Pick (9), in an early exposition of the concept of the schema, expressly acknowledges his indebtedness to Jackson.

may abolish some aspects of projection, leaving others intact. If a normal subject be pricked with a pin, he will recognize that this is so; the sensations are referred to the pin. In addition, the position of the pin will be more or less accurately localized. The force with which the pin is pressed against the skin will affect in a more or less simple and regular manner the intensity of the sensation. But these "spacial attributes, relative intensity and individual character" of the sensation are dependent upon the integrity of the cerebral cortex, whose function it is to mediate them. "When the influence of the cortex is removed", writes Head, "and the optic thalamus exerts its activity uncontrolled, the patient may cease to associate his sensory experiences with any external agency. He complains that he is being hurt, or that something is happening to him, but fails to recognize that he is being pricked with a pin. Removal of the cortical factors has reduced to elementary proportions the power of projection, as we know it in the intact human being. It is no longer possible to recognize the size, shape, weight, and spacial relations of an external object,¹ nor, indeed, to appreciate the relative intensity of the stimulating action it excites" (1, pp. 750-1).

Head believes the simplest projected aspect of sensation to be shown in the power to localize the site of a stimulated spot on the surface of the body. As in the case of postural recognition, this ability must, he maintains, presuppose a process of schematic relation. Moreover, appreciation of position may be lost without a corresponding loss of ability to identify the locality of the stimulated spot on the skin. For example, "... a patient may be able to name correctly, and indicate on a diagram or on another person's hand, the exact position of the spot touched or pricked, and yet be ignorant of the position in space of the limb upon

¹ Head points out that these attributes of sensation do not depend primarily on psychical functions. For he holds that, on the purely physiological level, "... afferent impulses possess projectional characteristics. The same class of impressions which underlie discriminative sensibility can be discovered at work controlling and regulating purely unconscious actions... Study of the reflexes at higher functional levels shows that they are governed by remarkably complex projectional relations... Thus... afferent impulses can be shown to be adapted to spacial conditions and to the intensity and relative character of the stimulus, although the whole procedure remains outside consciousness. If, however, they succeed in reaching the highest receptive centres, they endow sensation with spacial attributes, relative intensity, and individual character" (1, p. 751). Head nowhere states explicitly that at all levels projection involves schematic mechanisms and it will be remembered that the locus of these is in the level of the highest receptive centres. Bartlett, however, seems to have assumed that schemata operate on *all* functional levels (vide Part II of this paper).

which it lies". Head therefore concludes that this faculty of localization must be associated with the existence of another schema or model of the surface of our bodies.¹ This schema may, however, also be destroyed by a cortical lesion, and under such conditions the patient complains that he has no idea where he had been touched. "He knows that a contact has occurred, but cannot tell where it has taken place on the surface of the affected part" (1, p. 606).

Associated with projection by reason of its similar basis in the existence of the spacial schemata is the complementary function of *introjection*. This is the power of extending the recognition of posture, movement, and locality beyond the limits of our own bodies to the extremity of an instrument held in the hand, or otherwise attached to the body. Without the spacial schemata, Head writes, "we could not probe with a stick, nor use a spoon unless our eyes were fixed on the plate. *Anything which participates in the conscious movement of our bodies is added to the model of ourselves and becomes part of these schemata*; a woman's power of localization may extend to the feather in her hat" (1, p. 606; italics ours). In the same way, no one can drive a motor car effectively until intermediate processes by which his own bodily movements affect the behaviour of the car have been eliminated, and the vehicle has become a 'part' of its driver's 'body'.

Head's treatment of projection and localization is somewhat complicated by the introduction of his theory of protopathic and epicritic sensibility. On the basis of his classical experiment in nerve division, Head had advanced the theory that cutaneous sensation is dual in character, the two types being named by him epicritic and protopathic. While the epicritic is *par excellence* the system associated with projection, there is at the protopathic level an elementary form of this function. This is shown by the fact that when protopathic cutaneous sensibility alone is in operation "Any stimulus capable of evoking a cutaneous response aroused a widespread sensation, referred to some remote part

¹ This aspect of the schema theory has been elaborated at length in the writings of Pick (8, 9, 10). Pick appears to have developed his theory independently, but we are unable to ascertain whether he was in personal contact with Head before the appearance of Pick's first contribution in 1908. In his later writings, Pick adopted Head's conception of the schema in place of his own earlier notion of a 'spacial image' of the body. But he extended the theory to cover a much wider range of nervous and mental symptoms. In this, he has been followed by Schilder (11, 12). The notion of the 'body schema' (or 'body image') has, in fact, come to enjoy something of a vogue in contemporary neuropsychiatry. But the implications are so wide that they cannot profitably be discussed here. Adequate expositions of the general idea, together with much relevant clinical material, are to be found in the writings of Schilder (12), Tehehraz (14), and Lhermitte (5).

of the affected area" (1), p. 752). The sensation had the quality of extensiveness, although the amount of extension bore little relation to the actual size of the area stimulated. Thus, while the stimulus is wrongly localized, and its size incorrectly assessed, nevertheless it possesses in some degree those characteristics of determination which alone can make it relevant to speak of *error*. Moreover, "...this erroneous localization was not fortuitous, but remained the same whatever the stimulus employed..." (1), p. 752).

Head points out that this limited form of projection found in the protopathic system may be regarded as providing a primitive form of defence mechanism. It is essentially segmental in character, the sensation, erroneously localized as it may be, always being referred to some place in the same segment as that to which the stimulus is applied, and the response being in general one of compulsory withdrawal of the part concerned. It is only with the development of the *epicritic* projectional systems that choice of response is introduced. Thus in view of the supra-segmental character of these systems, flight of the organism as a whole becomes possible.

We have seen that in the epicritic projectional system sensation is endowed with those characters which allow discrimination and thereby permit choice of response. In addition, however, it is the function of this system to control protopathic activity. Involved too in projection is the capacity of the organism to relate sensation to external objects. An "object", Head writes, may in fact be defined as "a complex of projected responses". It is said to have characters such as size, shape, weight, and spacial position which distinguish it from all other objects. "The recognition of such features, however, depends on physiological activities, the product of certain definite centres in the cortex. If these processes are unable to influence consciousness, the 'object' disappears, although its affective and qualitative aspects still produce their appropriate sensory reactions" (1), p. 754).

We may infer that the afferent co-ordinations underlying epicritic projection are fundamentally schematic in character. For inasmuch as they confer upon sensation determination in respect of spacial attributes, intensity, etc., they involve comparison and relation to something that has gone before, or is co-present. "...all the higher projectional aspects of sensation...form a continuous series of dispositions determined by previous events of a like order. The unit of consciousness, as far as these factors in sensation are concerned, is not a moment of time, but a 'happening'. This consists in a group of occurrences belonging to

profoundly different orders in the psycho-physical hierarchy"¹ (1), p. 754).

Appreciation of temporal sequence represents yet another function attributed by Head to the existence of schemata. "The sensory activities of the cortex", he observes, "are not only responsible for projection in space, but also ensure recognition of sequence in time. For this would be impossible if a sensory impression had no necessary beginning or termination. One of the commonest defects produced by a cortical injury is this want of temporal definition; a stimulus, rhythmically repeated, 'seems to be there all the time'. The patient cannot appreciate the moment at which it is applied or removed. There is no complete recognition of an extended sequence of events" (1), p. 754).

Head therefore concludes that it is to the projected elements in sensation that we owe our conceptions of coherence both in space and time. These factors are not due essentially to "judgement or conscious association", but depend to a great extent on physiological activities and dispositions. "When these are permitted to excite consciousness", he concludes, "they appear as an ordered sensation, related to other events in the external world and extended serially in time" (1), p. 755).

'It will now be evident that there is more in Head's notion of projection than the mere association of a stimulus with an external object, or other unified source of causal influences. The different contributions made by what he calls projection to perceptual consciousness may, broadly speaking, be listed as follows in order of complexity: (1) The simple localization of a stimulus—that is, the reference of the present sensation to a causal origin having position in regard to a frame of reference. (2) Appreciation of the extensity of the stimulus—that is, the recognition that the causal origin of the sensation is extended in a certain way in a field of spacial relations. (3) The identification, or superposition, of the causal origins of a number of different sensations—that is, the recognition that two or more impressions emanate from the same source of causal influence, or from two or more sources localized in the same region and essentially interconnected. (4) The recognition in general that the whole external field of influences is structured by spacial and other relations. (5) Similar recognitions as (3) and (4) in

¹ The similarity to Kant is here especially apparent. In addition, however, to employing an essentially Kantian argument to the elucidation of the conditions of perception, Head is able to demonstrate the dependence of the perceptual characters in question upon the functional integrity of parts of the nervous system, and exhibit cases in which just these characters may be lacking.

regard to temporal sequence and relations. (6) In virtue of all these; appreciation of the 'object-character' of stimuli.

The true order of complexity of these functions of projection is, of course, a matter for further investigation. And it may be that they should not be separated according to the scheme suggested. Head himself says that simple localization on the surface of the body is the simplest of the functions, but does not trace their relationships further. And it is possible, for instance, that appreciation of 'object-character' may precede and not follow appreciation of the general structure of the environment.

It is important to make clear the dual role of schemata in elaborating the projectional aspects of sensation. On the one hand they have to provide the general frame of reference, or dispositional system, appropriate to each aspect.¹ Thus the localization of a stimulus is meaningless except in regard to a frame of spacial relations and this itself demands an enduring physiological mechanism. Perception of, and behaviour towards, the 'object-character' of a stimulus is similarly impossible in the absence of dispositional systems providing the general basis of such perception and behaviour. Incoming impulses from the periphery contribute, to the activity of the schemata in the cortex, the particular clues mediating the perception of the particular place in which a stimulus is localized. Similarly they mediate the perception of the particular 'individual character' of an object in the environment. But they cannot determine that the stimulus shall be localized at all, nor that an object shall be perceived as such. This, then, is the first function of the schemata, to provide permanent, yet continuously modified, physiological dispositions which, acting in co-operation with the immediate clues, can endow perception with the determinativeness of which we are in fact aware.² The second function is the active one of mediating particular perceptions so determined.

¹ We may note that C. S. Myers, as early as 1911, took over Head's concept of the schema in an interesting attempt to account for certain aspects of visual space perception; and, in particular, the auto-kinetic sensation (see (7), pp. 215-17, 231-2, 282-3, 294). Myers's approach has never been fully followed up, though Koffka has argued brilliantly on kindred lines in developing his theory of the 'spacial framework' (see Koffka(4), Chs. 5-7). These two approaches have much in common which would repay careful study.

² The schemata thus act *as if* concepts such as those of space and object were involved. This aspect of the functioning of schemata emerges again in Wolters's theory of conceptual thought and is discussed in Part IV of this paper.

(4) *Vigilance*

The term 'vigilance' was introduced by Head to denote a state of high-grade physiological efficiency of any given functional level of the nervous system. The state of efficiency of a given level is to be assessed by comparison with the maximum degree of adaptive function of which that level is capable. Thus in the *acute* decerebrate preparation there is a temporary lowering of vigilance, due to operational shock. This results in absence of the rigidity characteristic of the *chronic* state. When recovery has taken place, and the hind brain centres are again active in making their full contributions to the adaptive behaviour of the organism, namely the production of decerebrate rigidity, then the vigilance of this level is again said to be high. A high state of vigilance is associated not only with increased reaction but also with the optimal fulfilment of those functions delegated to the level of the nervous system under consideration. In the spinal cord these functions are those of reflex determination, and it is by the perfection, complexity and adaptive significance of the reflexes that can be elicited that its vigilance is to be assessed. In the hind brain the function concerned is that of affording the general basis of postural behaviour upon which may be imposed the niceties of adjustment afforded by the activities of the higher centres. Thus, in the decerebrate animal, the full development of rigidity is the expression of a high state of vigilance. The cortex is pre-eminently the organ of fine discrimination and consequently it is in the performance of the highest grade activities that its vigilance is displayed.

The vigilance of a given level of the nervous system may be reduced by a number of conditions. Fatigue is one; others are the toxic states produced by narcotic drugs and inflammatory conditions. Lastly there is structural damage. In this case, the lowering of vigilance is not restricted to mechanisms especially associated with the site of the lesion. The lowering of vigilance produced by cortical damage would seem to be comparable to the incidence of '*Diaschisis*' postulated by von Monakow,¹ at least in results, if not in mechanism. In any case change in vigilance is a physiological, and essentially a reversible, process (although, of course, the reverse process may occupy very considerable periods of time, and may never be completed).

¹ Head appears to think that this comparison is not truly valid, although his comments upon von Monakow's views are not very specific ((2), pp. 84-93). Von Monakow's conception involves a withdrawal from some parts of the cortex of facilitatory influences originating in other, now damaged, parts.

In virtue of the hierarchy implied in the Jacksonian concept of levels, it is evident that, where general "conditions unfavourable to physiological activity" subsist, a lowering of vigilance will first be apparent through failure in the highest grade performances of the organism. "The more highly differentiated the act, the greater degree of vigilance does it require and the more easily can it be abolished by toxic influences... or by other conditions unfavourable to physiological activity" ((2), pp. 486-7). There is thus a sense, employed by Head, in which the notion of vigilance can be applied to the organism as a whole. But it is important to bear in mind that, primarily, it applies to functional levels in the nervous system.

At the cortical level, lowered vigilance, whether it be due to general or local conditions, may inactivate, and even abolish, the postural schemata. "...If the vigilance of the so-called 'sensory' cortex is lowered, either temporarily or in consequence of organic destruction, not only will postural sensibility be disturbed in the appropriate parts of the body, but their static tone will be diminished. This is not due to loss of postural sensation, but to the destruction of those physiological processes on which they are based" ((2), p. 489). Thus, in general, if we exclude those cases in which the structural damage itself must be held directly responsible for the impairment of function, the abolition of postural schemata is to be envisaged as an *indirect* outcome of cortical injury. The *immediate* consequence of the lesion is reduction in that condition of high-grade physiological vitality called by Head 'vigilance'.

REFERENCES

- (1) HEAD, H. (1920). *Studies in Neurology*, vol. II. London: Hodder and Stoughton and Oxford University Press.
- (2) — (1926). *Aphasia and Kindred Disorders of Speech*, vol. I. Cambridge University Press.
- (3) JACKSON, J. HUGHLINGS (1931). *Selected Writings of John Hughlings Jackson*, ed. J. Taylor, vol. II. London: Hodder and Stoughton.
- (4) KOFFKA, K. (1935). *Principles of Gestalt Psychology*. London: Kegan Paul, etc.
- (5) LHERMITTE, J. (1939). *L'Image de Notre Corps*. Paris: Éditions de la Nouvelle Revue Critique.
- (6) MUNK, H. (1890). *Über die Functionen der Grosshirnrinde*, 2. Aufl. Berlin: Hirschwald.
- (7) MYERS, C. S. (1911). *Text-Book of Experimental Psychology*, vol. I, 2nd ed. Cambridge University Press.
- (8) PICK, A. (1908). *Über Störungen der Orientierung am eigenen Körper—Arbeiten aus der deutschen psychiatrischen Klinik*, Prag. Berlin.

- (9) PICK, A. (1915). "Zur Pathologie des Bewusstseins vom eigenen Körper." *Neurol. Zbl.* XXXIV, 257-65.
- (10) — (1921). "Störung der Orientierung am eigenen Körper." *Psychol. Forsch.* I, 303-18.
- (11) SCHILDER, P. (1923). *Das Körperschema. Ein Betrag zur Lehre vom Bewusstsein des eigenen Körpers.* Berlin: Springer.
- (12) — (1935). *The Image and Appearance of the Human Body.* Psyche Monographs, No. 4. London: Kegan Paul, etc.
- (13) SELZ, O. (1924). *Die Gesetze der produktiven und reproduktiven Geistestätigkeit.* Bonn: Fr. Cohen.
- (14) TCHÉHRAZI, E. (1936). *Image de Soi.* Paris: Librairie le François.

(Manuscript received 12 May 1941)

A SCIENTIFIC APPROACH TO MUSICAL AESTHETICS

BY CARL E. SEASHORE

- I. *Science can clarify and define essential concepts in aesthetics* (pp. 287–290).
- II. *Science broadens the horizon for insight into the full nature of the aesthetic situation* (p. 290).
- III. *Science creates a feeling of confidence in the tangibility of aesthetic issues* (pp. 290–291).
- IV. *Science aids in dealing systematically with aesthetic problems* (pp. 291–292).
- V. *The method of natural sciences is applicable in musical aesthetics* (p. 292).
- VI. *The method of physical sciences is also applicable in musical aesthetics* (pp. 292–293).
- VII. *Science in aesthetics proceeds by the operational method* (p. 293).
- VIII. *Science encourages co-operation with all other legitimate approaches* (pp. 293–294).

IN the current number of this *Journal* James Mainwaring has given a comprehensive criticism of the empirical approaches to aesthetics.¹ It is a clear presentation of a point of view often voiced and should be reviewed critically by the various interests concerned. Instead of attempting a formal criticism, let me set out a single constructive illustration of one empiricist's point of view in defence. In this I shall limit myself to a single concrete case dealing with the significance of a tonal spectrum in musical aesthetics from the laboratory point of view; and, for the sake of clarity and brevity, I shall limit myself to a few categorical statements which any competent investigator in the field of psychological acoustics can verify and evaluate in relation to the position taken by Mainwaring.

I. SCIENCE CAN CLARIFY AND DEFINE ESSENTIAL CONCEPTS IN AESTHETICS

The term 'tonal quality' represents one of the large categories of musical aesthetics. It has two components, namely, timbre and sonance. Timbre is a cross-section of a tone as represented by a single sound wave in terms of its harmonic structure, fundamental frequency and total

¹ *Brit. J. Psychol.* (1941), xxxii, 114.

intensity. Sonance represents the quality of a tone as determined by the harmonic structure of the successive waves in the tone as a whole. Thus timbre is a case of simultaneous fusion of partials at a given moment in the tone; whereas, sonance is a case of successive fusions of changing harmonic structures during the maintenance of a musical note. The present illustration will deal only with timbre, a term which, when understood in musical aesthetics, should be as useful as the terms pitch or rhythm. A parallel case could easily be made for sonance, thus covering the entire concept of tonal quality.

As a result of the recent extraordinary developments in acoustical techniques, the investigator can now select a fair sample of a note in its actual musical setting and take a highly detailed moving picture of every sound wave in that tone as it is generated. Realizing that everything that is conveyed from the performer to the listener is conveyed on sound waves, he has here the material basis for the analysis and clarification of all possible elements in the musical tone as an art object. The form of the sound wave is the physical basis of timbre. He can take as a fair sample any representative wave in the tone and run it through an harmonic analyser which will, in a single process, determine the number of partials present, their distribution, their relative formant grouping, and the amount of energy represented in each. These readings on a series of dials can be transferred into a table. The facts thus established can be represented in a single graph called a tonal spectrum.

He can convert this physical spectrum into a psychological or musical spectrum which, instead of showing the amount of energy in each partial, represents the audibility or loudness of each partial in terms of decibels. The art of making this transformation in accordance with the psychological and acoustic laws of hearing is a very recent contribution. Scientific aesthetics must employ both physical and psychological spectra; however, for the present purpose we shall think only in terms of the physical spectrum. From a spectrum the investigator can derive a complete, detailed, verifiable description and definition of the tone in cross-section, and in terms of the sequence of spectra he can give an account of the qualitative character of the tone as a whole. An experience of beauty should, of course, be couched in so far as possible in psychological terms. Likewise, the description of a physical tone should be couched in the established terminology of acoustics.

One of the greatest sources of confusion in aesthetics is abuse of language. In musical history we find scores of synonyms as substitutes for the term tonal quality, practically all loose and ill-defined. When we

once understand the nature and significance of the tonal spectrum as the basis of timbre and sonance, we can scrap all these inconsistent terms and thus establish a common usage among scientists, artists and philosophers.

To the scientific worker, the tonal spectrum becomes a tool as familiar as the photograph of a friend's face; a single glance at the spectrum immediately gives him a clear concept of the quality of the tone represented in cross-section. It may be as exact and as easily understood as a commonplace mathematical equation. When acoustic science has developed such technical terminology, that terminology soon tends to become popular so that the teacher or the artist as well as the composer can readily understand what a spectrum means, can formulate descriptions and definitions in terms of it, can criticize or instruct in tonal quality in terms of that picture, and can think and speak with scientific precision about the beauty or ugliness of a specific tone. For this purpose it is not necessary that he should go through the process of deriving the tonal spectrum any more than it is necessary for the pleasure driver of an automobile to be an expert in automobile engineering. Such transition from the purely technical concepts to popular and practical spheres is one of the splendid examples of growth in the spread of learning.

Current attempts to name tonal qualities have resulted in such crude terms as rich and pure, smooth and rough, big and small, but these are inadequate. We have such devices as speaking of the different vowel qualities, bearing in mind that the differences involved are differences in formant regions. Then in desperation we go one step farther and speak in terms of instruments, such as the flute tone, the trombone tone, or a violin tone, ignoring the fact that each of these instruments is capable of producing hundreds of differences in quality. All these devices are more or less beggarly. But if we proceed on the assumption that all differences in tonal quality are representable in the spectrum, we shall have made great progress in laying foundations for a consistent and permanent language of musical aesthetics.

Students of aesthetics may therefore fairly ask if it is not possible to discover or set up a consistent scale of tonal qualities named in terms of the variables of the tonal spectrum. We think this can be done when the facts are sufficiently in hand. For example, we could recognize as many types of spectra as there are letters in the alphabet and name them alphabetically. Then a person talking about tonal quality could refer in stabilized language to the spectra as A, B, C type on the analogy of names for vitamins or possible combinations of these, such as the

A-K type. Finally, descriptive names might be assigned to each for a given gamut of definable differences.

II. SCIENCE BROADENS THE HORIZON FOR INSIGHT INTO THE FULL NATURE OF THE AESTHETIC SITUATION

One of the objections recently made to the experimental method is that the laboratory experimenter raises more questions than he can answer. That is one of the great merits of the scientific method. The experimental psychologist would go beyond the prevailing adage, that if you ask one question of nature nature will ask you ten, and say he can set up a sample experiment in aesthetics which will raise a hundred other relevant questions not previously thought of. When Wundt was asked what he had learnt from the reaction-time experiment, he replied that it had given him an entirely new concept of the human mind. So I would say that while the experimentalist in the laboratory can give a final and verifiable solution to only one very minute problem at a time, one of the great merits of the experimental procedure lies in the fact that it forces upon the horizon a vast array of issues which come as corollaries to the situation solved. It is obvious that recent progress in experimental psychology has performed this very function for aesthetics by generating a deeper insight into the nature of experience and expression of aesthetic emotions, and the nature of the relation between the aesthetic object and one who creates or feels beauty or ugliness in it. Would it be unjust to say that the laboratory empiricist is to the non-empiricist in aesthetics as the astronomer is to the stargazer? Both look into the starry heavens but they see entirely different worlds of beauty.

III. SCIENCE CREATES A FEELING OF CONFIDENCE IN THE TANGIBILITY OF AESTHETIC ISSUES

Many of the serious books on aesthetics now in our libraries take the same attitude toward aesthetic experiment that the forerunners of modern psychology took toward psychological experiment a hundred years ago: mind is so different from matter; beauty is such an ethereal and fleeting situation; a feeling of beauty is never constant; after all, beauty deals with subjective values, not objective facts; problems that can be solved in the laboratory are only an infinitesimally small aspect of the aesthetic situation; like the historical soul which was not subject to experiment, beauty is a sort of ultimate reality in itself, intangible.

But when the theory of evolution came upon the horizon, there was a sudden about-face in philosophical psychology and experimental psy-

chology became an inceptive science. Fechner and his followers in experimental aesthetics committed many a blunder, but they made progress through trial and error which resulted in the gradual acceptance of the feasibility of scientific experiment in aesthetics.

The critic must distinguish between empirical and experimental. Great discredit is rightly thrown-upon aesthetics by the publication of empirical studies of beauty which do not conform or measure up to the sanctions of scientific procedure. They bring discredit upon scientific method, as, for example, in the case of securing judgements about likes and dislikes of presumably beautiful objects without setting any experimental control to determine on what feature the aesthetic judgement is based, in judgements which ignore the influence of the total situation in which the experience occurs, in questionnaires which are certainly subversive of scientific procedure, and in the statistical treatment of uncountables. But such are the fumbings not only in an inceptive applied psychology but also in an inceptive art or philosophy.

However, none can dispute that a fundamental tool has objective validity as in the case of the tonal spectrum. The student of musical aesthetics must take notice, and what is more, develop a faith and confidence in objective and verifiable procedures. Twenty years ago I despaired of the possibility of seeing in my lifetime a relatively exact science of tonal quality. But modern acoustics has been revolutionary in its progress, and our present equipment in the laboratory now enables us to face that problem with enthusiasm and feelings of certainty in findings.

IV. SCIENCE AIDS IN DEALING SYSTEMATICALLY WITH AESTHETIC PROBLEMS

Take again the problem of the sources of beauty or ugliness in tonal quality as represented by the spectrum, and consider one of the elements, such as the formant, that is, the character of the grouping of dominant partials. Analysis of this problem shows that the principal variables in the formant are the number of formants present, the position of the formant, the width of the formant, and the relative prominence of the formant. These are indisputable facts which must be taken into account when making aesthetic judgements about tonal quality. And the subdivision of factors involved must be carried further. Thus the element of dominance of formant regions in musical tones can be fractionated so as to determine what qualitative characteristics of the tone are due to specific types of distribution of formant regions. This is now illustrated

in acoustical studies of speech where vowels are defined in terms of characteristic formant regions. This method is now employed in comparative studies of musical instruments. Progress is slow but in firm step so that any one who teaches, thinks, or creates intelligently in terms of tonal quality should understand the structure and function of formants. Composers are becoming alarmed at their growing responsibility for formants, both objective and subjective.

V. THE METHOD OF NATURAL SCIENCES IS APPLICABLE
IN MUSICAL AESTHETICS

In a virgin territory, the botanist collects as many and varied specimens as he can find and then takes them into his laboratory, examines each in detail, and names and classifies them. So the student of aesthetics may collect specimen features of beauty or ugliness in actual musical situations, and examine, name and classify them. Take again our problem of timbre. Each specimen is represented by a spectrum. No botanist has collected and classified all plants, but at one time botanists began to do so and acquired confidence in their ability. No one will ever collect samples of all varieties of beauty in the quality of musical tones, but each investigator and each generation will go as far as possible under the limitations of time and the extent of facilities. Preceding science has given him the tools, such as the art of determining the spectrum and the insight into the nature and ramifications of its significance.

VI. THE METHOD OF PHYSICAL SCIENCES IS ALSO APPLICABLE
IN MUSICAL AESTHETICS

The sustained illustration which I am carrying through is an example of the use of physical method in so far as we are dealing with the nature and significance of musical sounds. We can not only make physical analysis of specimen sounds taken from actual music, but we can reverse the process and build synthetic tones on the specifications for any harmonic structure. We now have in the laboratory a tone generator capable of sounding tones of any desired harmonic structure composed of the first sixteen partials or less. The operator at the panel can determine how many partials shall be present, the character of their distribution, the amount of energy to be assigned to each, and the phase relationships of partials. Then he presses the button and out comes the tone of the structure specified. With so many variables, the mathematician tells us we could produce more than a million varieties of tone, thus imitating voice, musical instruments, sounds in nature or any other theoretically

desired kind of tone. Thus we proceed in the opposite direction of the naturalist and produce tones synthetically in order to determine the range and types of recognizable and significant elements of musical beauty.

VII. SCIENCE IN AESTHETICS PROCEEDS BY THE OPERATIONAL METHOD

As a sequel to the fractionating of problems for the purpose of the experiment, psychologists profitably take an operational point of view; that is, they state the circumstances under which the observation was made in such a way that the conditions can be repeated, and they limit their primary conclusions to those factors which were actually under control. The first step is to show what the actual experiment is in this particular case. If we are then interested in whether or not that is regarded as a relatively good or poor spectrum, we would have to resort to further experiment with aesthetic judgements. If we want to generalize, we would have to make a corresponding series of measurements on the principal variables which may enter; and if we want to study the variability in the aesthetic value of this particular tone, we would have to experiment with some of the principal variables such as age, training, artistic temperament, or what the observer had for breakfast. In other words, the progress of experimental aesthetics is exceedingly slow and is limited primarily to the establishment or verification of basic facts.

VIII. SCIENCE ENCOURAGES CO-OPERATION WITH ALL OTHER LEGITIMATE APPROACHES

Under the preceding headlines I have attempted to state the point of view represented by a psychologist in musical aesthetics in his attempt to interpret scientific approaches. Other experimenters in this field might and do differ from this point of view, but I submit it as a fair sample in the light of which the validity of the assumptions and criticisms of Mainwaring might be reviewed.

There are many problems in aesthetics which should be approached mainly from the philosophical point of view, such as theories of aesthetic value or the nature of beauty as reality. One must also take cognizance of the inspirations of the mystic. The splendid progress made in creative music has furnished the most basic groundwork we have for the determination of aesthetic values, particularly as dealing with problems and possibilities of design in musical form. The artist's interpretation of the

printed score is, of course, the commonest object of aesthetic judgement. Musical anthropology envisages the racial development of art principles. Musical history is a critique of unfolding art principles. Genetics traces the rise of aesthetic judgements in the growth of the individual. Education develops methods for training in aesthetic judgement. The experimental sciences unravel new facts and can put existing theories and practices to the acid test. The pursuit of beauty is fascinating and profitable at each of these fronts.

Why not encourage all such approaches? Let the musical historians, anthropologists and critics enlarge their storehouses and submit to periodic house-cleaning by separating the valid material from the invalid. Let the composer enlarge our conceptions of beauty by adding to our heritage of musical creations as art objects. Let the educationist put evolving theory into practice. Let the philosopher tell us from time to time what the greatest thinkers have thought about the nature of beauty and aesthetic values. Let the experimenter be ever ready and challenge all comers to verify and validate their facts and theories. Let us clarify the concept of beauty progressively by requiring each contributor to define beauty from his area, his purpose and his point of view.

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THE APPRECIATION OF HUMOUR: AN EXPERIMENTAL AND THEORETICAL STUDY¹

BY H. J. EYSENCK

From the Psychological Laboratory, University College, London

- I. *Introduction* (pp. 295-296).
- II. *Plan of investigation* (pp. 296-297).
- III. *Experimental results* (pp. 297-301).
- IV. *Temperamental factors* (pp. 301-302).
- V. *Theories of humour* (pp. 302-307).
- VI. *Summary and conclusions* (pp. 307-308).
- References* (pp. 308-309).

I. INTRODUCTION

IN the investigation reported here, an effort was made to deal with three questions in the field of humour by means of the method of factorial analysis. The first of these questions concerns the relative generality (or objectivity) of the appreciation of humour. Some investigators, such as Heim⁽²⁾, report little or no agreement among the subjects taking part in their experiments with regard to the 'funniness' of the jokes used as stimuli; others, such as Stump⁽³⁾ and the authors of the Roback, Moss, and Allcock tests of humour, explicitly or implicitly assume a comparatively high degree of agreement. No quantitative answer, stating the actual amount of agreement with reasonable exactitude, would appear to have been given to date.

The second question relates to the different types of appreciation involved in responses to humorous stimuli. While a great number of such types have been suggested by various authors, others have maintained that any attempt to discover such types "would merely involve a listing of individual jokes and individual people" (⁽²⁾, p. 161). Here also, a quantitative answer showing clearly the relative importance and influence of any type factors would appear desirable.

¹ This paper reports part of the work carried out by the writer during his tenure of the John Stuart Mill Research Scholarship at University College, London. Several researches of a similar nature have been, and are being, carried out at University College, and the writer wishes to record his indebtedness to Prof. C. Burt for valuable suggestions and criticisms. Acknowledgement is also due to Dr H. Babington-Smith for her kindness in giving the tests described below to a number of Scottish subjects, and to Prof. C. Landis, who very kindly allowed the writer to use his original material in this investigation, and who answered several queries relating to his own experiment^(a).

The third question to be raised concerns the influence of temperamental factors on the appreciation of humour; more particularly, it appeared possible that the 'Personality Factors' isolated by Guilford⁽¹⁵⁾ might to some extent determine the subjects' reactions to the various tests. An investigation of this possibility constitutes the third part of our experimental study.

Lastly, the experimental results gathered in the course of the investigation, and the subjects' comments and introspections, appeared to throw a certain amount of light on the vexed problem of the theory of humour and laughter, and it seemed worth while to record briefly the main conclusions to which the data seemed to point.

II. PLAN OF INVESTIGATION

Three humour tests, containing respectively 100, 52, and 37 items, were given to a group of sixteen subjects, the instructions being in each case to rank the items in each of the tests in order of 'funniness'. Eight of the subjects were University students; the other eight were entirely unconnected with academic life. The distribution of the sexes was equal. Ages ranged from 17 to 35. Introspections of the subjects were in some cases taken down in shorthand, while in other cases the subjects themselves wrote down valuable comments. The number of items in each of the three tests was too large to make straightforward ranking possible, and a group-ranking procedure had to be employed. By this method, marks are given to the various items in accordance with a prearranged system, the same mark being given to a fixed number of items. The distributions used followed as closely as possible the normal probability curve.

The tests were chosen with a view to covering three comparatively distinct varieties of humour, viz. verbal jokes (to be called 'jokes'), humorous drawings with captions (to be called 'pictures'), and the humour resulting from the opposition of two photographs showing certain points of similarity while being entirely different in other respects (to be called 'comparisons'). As an example of this last type of humour, the opposition of a photograph of Laval to one of a toad may be quoted, where there is a notable similarity of attitude and expression. Collectively, the items in the three tests will be called *jokes*.

The verbal jokes used were identical with those used by Landis & Ross in their investigation⁽¹⁾; their selection was carefully made so as to contain roughly equal proportions of good, medium and bad jokes, and to contain representative examples of the seven types of humour

recognized by Landis and Ross in accordance with the theoretical analysis of 'sense of humour' given by Eastman (4).¹ These seven types of humour are defined as follows:

(1) *Humour of Quantity*. Results primarily from obvious exaggeration (over- or under-statement of facts, thoughts, etc.).

(2) *Humour of Incongruity*. Results primarily from the association of two generally accepted incompatibles.

(3) *Humour of Unexpected*. Results primarily from the occurrence of some surprising fact, thought, feeling, etc.

(4) *Humour of Truth*. Results primarily from projection of self into situation, with consequent exposure of unrevealed thoughts.

(5) *Humour of Superiority*. Results primarily from the inability of others to handle adequately situations which to us seem simple.

(6) *Humour of Repression*. Results primarily from the release of tension aroused by thoughts, feelings, etc. of sex, fear, etc.

(7) *Humour of Ridiculous*. Results primarily from obviously nonsensical use of logic, verse, etc.

The 'pictures' were selected by the present writer on the same principles from several thousand drawings which had appeared in *Punch*, *Lilliput*, *Everybody's*, *Illustrated*, *Razzle*, *The Humorist*, *College Humor*, and *Movie Humor*. The 'comparisons' were taken exclusively from the pages of *Lilliput*, and an effort was made to include examples of all the different types of 'comparisons' appearing in that journal.

Two controls, or 'jokers', were introduced into the 'pictures' and the 'comparisons' tests in order to test certain assumptions regarding the influence of the behavioural field. These controls, while similar in form to the other items in the tests, were made up in such a way that they were not funny at all, but quite meaningless: in the 'pictures' test, the captions were cut off two drawings, and replaced by quite unrelated captions, and in the 'comparisons' test two comparisons were made up by putting together entirely unrelated photographs.

III. EXPERIMENTAL RESULTS

In Table 1 are given some of the numerical results of these experiments. Two statistically significant factors were extracted from the correlations between the rankings of the items in each of the three tests;

¹ This analysis is quoted below in some detail because it is typical of many other attempts. The present writer is dubious about its value as a psychological account, but it would appear that in the absence of a better analysis it may be of help in making sure that no important type of humour is overlooked in the selection of jokes.

from the 'pictures' test a third factor was extracted whose significance is doubtful. The numbers of correlations exceeding Fisher's $p=0.05$ and $p=0.01$ levels in each of the tests are shown below as 'significant correlations' and as 'very significant correlations' respectively.

Table 1

	Jokes	Pictures	Comparisons
Average intercorrelation	0.14	0.20	0.15
First factor variance	16.5 %	22.1 %	19.6 %
Second factor variance	6.2 %	5.6 %	7.0 %
Third factor variance	—	5.2 %	—
Significant correlations	32	34	30
Very significant correlations	18	17	9
Highest $+r$ in table	0.37	0.55	0.58
Highest $-r$ in table	0.17	0.08	0.45
Percentage of jokes found funny	35	44	51

These results enable us to answer our first query: How much generality or objectivity is there in each of the three tests employed, for the sample of the population tested? The answer is given by the percentage contributed to the variance by the first, positive, general factor: 16.5 % in the 'jokes' test, 22.1 % in the 'pictures' test, and 19.6 % in the 'comparisons' test. As these values are comparatively close together, we can take their average, 19.4 %, as representative. This value is very similar to the average of the first factor variances for eighteen tests of aesthetic appreciation reported by the present writer⁽⁵⁾, which reached the value of 19.9 %. The average of the intercorrelations in these eighteen aesthetic tests was 0.171, while in the three humour tests it was 0.163. Thus agreement on the relative 'beauty' of the 280 pictures used altogether in the aesthetics investigation was almost identical in degree with the amount of agreement found in this experiment with regard to the relative 'funniness' of 189 jokes.

In the aesthetics experiment just mentioned, it was found that those observers who had high saturations in one test tended to have high saturations in other tests also. The correlations between the subjects' first factor saturations for the three humour tests used in the present investigation were found to be all positive, but not statistically significant. (According to Fisher's test for small samples, a correlation of 0.50 would be significant ($p=0.05$) and a correlation of 0.62 would be very significant ($p=0.01$) when the number of items correlated is 16.) The actual correlations are 0.17 ('jokes' and 'pictures'), 0.26 ('jokes' and 'comparisons'), and 0.05 ('pictures' and 'comparisons'); the average of these correlations is 0.16.

There is a marked tendency for the person who finds a large number

of jokes funny in one test to find a large number of jokes funny in other tests also. The correlations between the tests are 0.66, 0.58, and 0.41, averaging 0.55. While it is probable that these correlations are to some extent due simply to different 'levels of aspiration' in the subjects, the introspections show that there is quite a genuine difference between them as regards the number of jokes found at all amusing. There is a slight but constant positive correlation between liking a large number of jokes and having a high factor saturation for the general factor; the correlations are 0.11 ('jokes'), 0.47 ('pictures'), and 0.08 ('comparisons'), averaging 0.22.

As regards the question of type factors in the appreciation of jokes, the most definite and clear-cut of these factors is found in the 'pictures' test. This bipolar factor, which contributes 5.6 % to the variance, divides those observers who show a certain preference for sexual jokes from those who put jokes of this kind comparatively low in their rankings. Neither sex nor age seemed to have any very obvious influence with regard to this factor. No sexual factor occurred in the 'comparisons' test because the material used contained no sexual jokes. In the 'jokes' test, where a few of the items were slightly sexual, a third factor extracted from insignificant residuals seemed to indicate a similar dichotomy between liking for sexual jokes, and dislike for them, but the dichotomy was not marked enough to permit of definite interpretation.

Interesting defence mechanisms appeared in some cases when subjects who did not like sexual jokes offered some far-fetched interpretations of the joke which left out its obvious sexual significance, or just refused to understand it at all. This appears to be in good agreement with Ghosh's finding that "in some cases, factors have been overstressed or even read into jokes, while other factors were neglected or overlooked in order that the jokes might be made to fit into the pattern of the subjects' mental states" (6). While this misunderstanding of jokes did not occur exclusively where sexual jokes were concerned, it seemed to occur most frequently in these cases. Sexual jokes seem to fall largely into Eastman's categories of 'Humour of Truth' and of 'Humour of Repression'.

The third, bipolar factor extracted from the 'pictures' test indicates an opposition between one group of subjects who prefer jokes which are based mainly on the *character* of the persons depicted (personal aspect), while another group prefer jokes which are based mainly on the humour of the *situation* (impersonal aspect). This factor seems to be closely related to the personal-impersonal factor found by Kambouropoulou in her analysis of diary entries, joke tests, etc. (7). One might feel tempted

to relate this factor to the familiar opposition between formal and representative art; the impersonal type of humour is created by the formal aspects of the situation, while the personal type depends on the representative character of the people depicted.

The second, bipolar factor in the 'comparisons' test, which contributed 7.0 % to the variance, opposes one group of people who tend to prefer photo-comparisons in which one of the two pictures is amusing by itself, without taking into account the relation with the other picture, to another group who place great stress on this relation, without paying much attention to the amusing nature of the individual pictures. We may in this connexion recall a similar factor found in the preference rankings of colour combinations; some people judge almost entirely by the individual colours in the combination, while others base their judgement largely on the relations between the colours (8, 9). This opposition between the group who preferred the complex, relational 'comparisons' and those who preferred the simpler ones became apparent also in their comments; the 'comparisons' depending for their effectiveness on the relation between the pictures were often called 'clever', while the other comparisons tended to be called 'funny'. Martin, in her pioneer work, noted the same opposition (10, p. 39), and it seems to have played an important part also in the distinction drawn by Hollingworth between 'waxing' and 'waning' jokes (11).

The second, bipolar factor in the 'jokes' test, which contributed 6.2 % to the variance, appears to be related in some measure to the factor discussed above. Here also one group of subjects like the simple, straightforward, 'funny' type of joke, while the others prefer the more complex, 'clever' sort of humour, in which a comparatively large number of relations requires to be deduced. It is interesting to note that a similar difference has been found to play an important part in aesthetic preference judgements: a complexity-simplicity factor was found in work on preferences for poetry (12) and for polygonal figures (13).

The controls or 'jokers', mixed in with the ordinary 'pictures' and 'comparisons', occupy on the average the last two places in the ranking orders. Several subjects, however, put them relatively high in their rankings, in two cases awarding them next to the maximum number of points. This shows how easy it is to create artificially the desired ego-object relation, and how efficient an artificially created relation of this kind can be in changing the behavioural field of the subjects.

There was no differentiation of the sexes in any of the factors discussed above. This result is in good agreement with Heim's finding, who

discovered "a surprisingly high degree of agreement between men and women" (2), p. 155). She discounted her own finding as "probably untrustworthy", giving no reason beyond saying that "everyone undoubtedly believes that fundamental and universal differences exist" (ibid.). In the absence of proof for the existence of such differences, it would appear safer to rely on the experimental results, instead of on popular conviction.

In view of the fact that the number of subjects taking part in this investigation was relatively small, the question may be raised of just how much reliance can be placed on the results. Some aspects of this problem have been discussed elsewhere⁽¹⁴⁾, and when the formulae quoted there are applied to the results summarized above it is found that the correlation of the average order given by our subjects for the items in the three tests would correlate with the true order of the whole population of which they are a sample to the extent of approximately 0.90. High values of this kind would appear to lend a certain degree of validity to our conclusions.

IV. TEMPERAMENTAL FACTORS

In an attempt to obtain a picture of the subjects' temperamental peculiarities, they were asked to rank in order of 'applicability' twenty-five temperamental traits, typed on separate slips of paper. That is to say, they were instructed to put the trait which they considered most characteristic of themselves at the top, the one they considered least characteristic at the bottom, and the others in between in order of applicability. The traits were taken from Guilford's research into personality factors *S*, *E*, and *M*⁽¹⁵⁾, by choosing the traits with the highest saturations for these three factors.

The rankings of these traits were then correlated, and the table of intercorrelations factor-analysed. Two factors, both of which were significant, were extracted, accounting for 30.0 and 11.1 % of the variance respectively. Both factors were bipolar, and while the first factor seemed to differentiate between the aggressive, masculine type of person, and the emotionally dependent type, the second factor distinguished between the sociable and the unsociable. In other words, factor I opposed Guilford's traits *M* and *E*, while factor II opposed the two aspects of his trait *S*.

Several correlations of these two temperamental factors and the type factors isolated earlier in this study approached or reached statistical significance. The sexual factor correlated to the extent of -0.48 with

the emotionally dependent-aggressive factor, and to the extent of -0.42 with social shyness. This connexion between the liking for sexual jokes and an aggressive, not emotionally dependent, sociable kind of temperament is perhaps not contrary to what one would have expected on a priori grounds, and is of particular interest in view of Ghosh's findings (6). It may also be noted as giving support to certain orectic theories of humour and laughter which will be discussed in the next section.

The complexity factor in the 'jokes' test was found to correlate 0.16 with the emotionally dependent-aggressive factor, and 0.61 with the factor of social shyness. The complexity factor in the 'comparisons' test correlated 0.21 with the emotionally dependent-aggressive factor, and 0.21 with the factor of social shyness. As both social shyness and emotional dependence are generally regarded as introverted characteristics, while sociability and aggressiveness are extraverted characteristics, we are perhaps justified in the conclusion that introverts tend to prefer complex jokes, while extraverts tend to prefer simple jokes. (Similarly, introverts tend to dislike sexual jokes, while extraverts tend to like them.) While the correlations on which these conclusions are based are not always significant, it should be noted that they all point in the same direction.

It may be of interest to note here that the connexion between introversion and liking for more complex stimuli does not seem to be restricted to the field of humour. In an investigation into the factors determining the appreciation of poetry, a similar connexion has been noted (12), and from the references quoted there it will be seen that a similar relation between temperament and aesthetic preferences obtains also in other fields.

The correlations between the personal-impersonal factor, and emotional dependence and social shyness, are too low to be of any significance, being only 0.05 and 0.15 respectively.

While the conclusions stated above are suggestive rather than definitely established, it seems certain that temperamental traits influence the appreciation of jokes, just as they have been shown to influence the appreciation of poems, pictures, etc. It would appear likely that further experimentation involving other personality factors would throw considerable light on the genesis of laughter.

V. THEORIES OF HUMOUR

When the theories of humour advanced by the one hundred or so best-known authors are examined, little agreement is found on any of

even the most fundamental points. This disagreement is due partly to the fact that most authors in the past have attempted to relate their theories of humour to their general philosophical theories, thus introducing into the former many of the questionable assumptions and controversial views of the latter; it is due also to the fact that few philosophers have given much attention to the fundamental question of exactly what are the stimuli which cause laughter.

Again and again, the hat that is blown off by the wind, or the fall in the street, are quoted as typical mirth-provoking instances; thus Bergson begins a famous passage in his book by saying: "A man, running along the street, stumbles and falls; the passers-by burst out laughing" (16). A numerical count by the present writer of seven occasions when four men and three women stumbled and fell, and of five occasions when the wind blew off the hats of two men and three women, did not reveal a single instance of laughter among the total of more than 100 passers-by. Facts such as these must make us suspicious of all theories which might explain after a fashion why laughter should occur, but which fail to explain why actually it does not occur. Similarly, philosophical theorists frequently posit certain feelings of superiority or of expectation which those who laugh are supposed to experience in certain circumstances, but which careful introspection often fails to discover.

When the actual theories which have been suggested to date are surveyed, we find such a variety that it is difficult to discover an underlying basis according to which they might be classified. To a psychologist, the most satisfactory classification is perhaps one which follows the usual division into cognition, conation, and affection, and indeed most theories seem to fall quite naturally at some point in a triangle whose three corners are marked as in Fig. 1.

Most numerous of all are those theories which stress such cognitive elements as incongruity, contrast between ideas, deceived ideational expectation, and the like. The long list of writers who have held such theories contains among others the names of Cicero, Quintilian, Dryden, Locke, Marmontel, Gerard, Campbell, Beattie, Priestley, Kant, Jean Paul, Hazlitt, Brown, Schopenhauer, Everett, and those who, following Spencer, introduced the added requirement that the incongruity should be descending—Lipps, Sidis, Marshall, and Renouvier and Prat. Schiller and Willmann may be quoted as modern champions of some form of cognitive theory, and so may Maier.

Almost equally numerous is another group of writers who stress the conative aspect of laughter, relating it to the satisfaction of the desire

for superiority, or 'self-glory' as Hobbes has it. In this class are, for instance, Plato, Aristotle, Trissino, Hobbes, Hegel, Laménais, Hunt, Bain, Philbert, Michiels, Carus, and Bergson. Chandler(17) and Kimmins(18) have provided a certain amount of experimental proof in favour of this theory, which finds its latest champion in Ludovici(19). Following

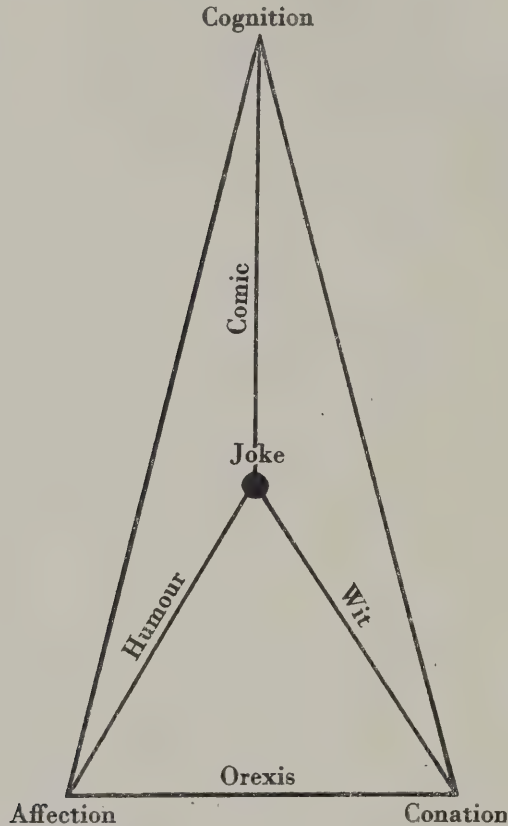


Fig. 1. Diagram representing the structure of the joke, showing the three-fold determination of laughter by cognitive, conative, and affective factors.

Wrench(20), he has suggested the term 'superior adaptation' as characterizing all instances of laughter, such laughter in his view being due to the consciousness of superior adaptation on the part of the person laughing.

The affective aspect of laughter is stressed by those who have directed their attention more to the emotional component of laughter. This is usually conceived to be pure joy, or else joy in combination with some

other emotion, such as fear or anger. Alternatively, a contrast of feeling is posited as being essential to laughter. Joubert, Descartes, Hartley, Laprade, Dumont, Höffding, and McDougall may be mentioned here.

Some writers cannot be placed near one corner of our triangle, but must find their place along one side; thus Ribot, Sully, and Santayana have advanced theories which recognize both the cognitive and the conative aspects of humour. Freud may be said to have recognized all three aspects to some extent; his definition of wit as being due to an economy of expenditure in inhibition stresses the conative aspect, his definition of the comic as being due to an economy of thought stresses the cognitive aspect, and his definition of humour as being due to an economy of feeling stresses the affective aspect. His theory is vitiated, as Eastman points out, by his uncritical acceptance of the mechanical Spencer-Lipps theory of 'economy', which is really foreign to the remainder of his views. He may also be criticized for making too rigid a distinction between the three different kinds of laughter, i.e. that of wit, of humour, and of the comic; the mind acts as a whole, and in every case of laughter all three components must play their part, although their relative importance may vary from case to case.

In considering these three divisions, it appears that we can with advantage group together the conative and the affective aspects, under the general name of 'orectic'. The reason for this grouping is of course that these two aspects are related much more closely to each other than either is to the cognitive aspect. If we follow Ludovici in his well-documented account, we may define the orectic component of laughter as being due to the 'joyful consciousness of superior adaptation'. We cannot follow that author, however, in neglecting entirely the cognitive aspect, particularly as it is this component which is most noticeable in the type of material used in the present investigation. Before turning to an analysis of this cognitive component, we may note certain facts regarding the relative influence and importance of the orectic and cognitive factors.

Ludovici's own data seem to show that historically and genetically we have a progress from the laughter of superior adaptation, as shown for instance in the laughter at physical imperfections and at accidents, to the laughter at the intellectual joke, which is almost purely cognitive (19, pp. 88-103). Even in the realm of the joke, however, we find the opposition between the orectic and the cognitive aspects; the contrast between 'funny' and 'clever' jokes, noted in a previous section, appears due to the relative preponderance of orectic or cognitive factors. That

this is a true interpretation is indicated by the correlation between aggressiveness and liking for simple, 'funny' jokes. The correlation between liking for sexual jokes and aggressiveness seems also to be due to the orectic factor. These results seem to indicate that the view often held, viz. that introverts have less sense of humour than extraverts, is false, and that we should say, rather, that introverts have an appreciation of humour in some ways different from that of the extraverts. More particularly, while in extraverted persons the orectic aspect of humour is paramount, in introverted persons the cognitive aspect is supreme.¹

While apart from the correlations just mentioned there is little in the results of this research to help in a discussion of the orectic components of laughter, our material is more extensive with regard to the cognitive aspect. When the experimental material is examined with particular reference to the 'funniness' of the items, as revealed by their average position, and to the introspections and comments of the subjects, certain conclusions emerge which seem to be helpful in an analysis of the cognitive factor. The majority of the points noted are not new, and have indeed recently been discussed by Maier⁽²¹⁾ and Willmann⁽²²⁾ in their interesting theoretical contributions, but in one or two places there are certain amplifications and modifications which appeared to be essential in view of the experimental results.

(1) In the first place, we find that each one of the jokes used contains two or more ideas, attitudes, or sentiments between which there is a certain amount of *contradiction* or *incongruity*.

(2) Secondly, we find that these contradictory ideas, attitudes, or sentiments are fused, united, or *integrated* to a certain extent during the course of the joke, and that laughter results when this point is reached.

(3) Thirdly, this fusion, union, or integration is *sudden*; when it is protracted, the joke arouses no laughter and loses its point.

(4) Fourthly, the process of integration is accompanied by *insight*; as Maier says, there is a change in the meaning of the elements of the humorous experience. The total configuration of ideas, attitudes, or sentiments built up in the first part of the joke is changed, and we gain an insight into the structure of the total field which previously was lacking.

(5) Fifthly, the elements contained in the joke must be experienced

¹ In support of this view, it may be noted that none of the correlations between the temperamental factors and the first, general, positive factor in the humour tests even approached significance. On the customary view, the extraverted traits ought to be correlated positively with this general, positive factor. The complete absence of any such correlation favours the view outlined above.

objectively, not emotionally. (This demand, of course, is the counterpart to the demand for 'psychical distancing' in aesthetics.) This condition does not eliminate the occurrence of an emotion of joy as a result of the joke, but merely prevents any emotional attachment to the elements contained in the joke.

Taking these five points together, we may say that on the cognitive side, *laughter results from the sudden, insightful integration of contradictory or incongruous ideas, attitudes, or sentiments which are experienced objectively*. Other things being equal, the funniness of a joke is a direct function of the degree of contradiction or incongruity between the main ideas, attitudes, or sentiments contained in it, and the quality of the integration of these elements, as measured by the suddenness of, and the degree of insight resulting from, this integration.

- Common

While it is permissible for theoretical purposes to isolate the cognitive aspect of humour in this fashion, it should not be forgotten that in each particular case of laughter the orectic aspect too must be considered, even though in experiments of the kind described here the influence of this factor is reduced to a minimum. In general, there will be little difficulty in deciding in each case of laughter where in our triangle its main cause ought to be looked for, and the usefulness of such an eclectic theory as that presented here will not be diminished because it may prove impossible to find many causes of laughter which can be placed exactly at one corner of the triangle; it seems obvious that the great majority will be found somewhere inside the figure, the exact position being determined by the relative importance of the three factors we have distinguished.

VI. SUMMARY AND CONCLUSIONS

Three tests involving the ranking of altogether 189 jokes of various kinds were given to sixteen subjects. The resulting rankings were correlated, and the tables of correlations factor analysed. A test of temperament was also given.

In each of the three analyses, a positive, general factor appeared first, accounting on the average for 19.4 % of the variance. Several bipolar factors were also extracted, each of which contributed some 5-7 % to the variance. These factors divided the subjects into types according to the following principles of classification: (1) liking for *sexual* as opposed to *non-sexual* jokes; (2) liking for *complex* as opposed to *simple* jokes; and (3) liking for *personal* as opposed to *impersonal* jokes.

Extraverts were found to prefer sexual and simple jokes, while intro-

verts preferred complex and non-sexual jokes. It was suggested that the assertion often made which attributes a better sense of humour to the extraverts than to the introverts is wrong, and that we are dealing rather with differences in the manner of appreciating humour than with differences in the amount of sense of humour possessed by the two types.

Analysis of the jokes used, their relative positions in the average ranking order, the introspections and comments of the subjects and the various correlations between types of appreciation and temperamental factors, led to a theory of humour which stressed the complex nature of the phenomena investigated. On the orectic side, the results supported the view that laughter was due to the joyful consciousness of superior adaptation, while on the cognitive side the conditions responsible for the emergence of laughter could be summarized under five headings which emphasized the sudden, insightful integration of contradictory or incongruous ideas, attitudes, or sentiments which are experienced objectively.

The distinction between the orectic and the cognitive aspects of laughter was declared to be useful in theory, but the warning was added that in practice *both* factors are usually active, in varying proportions, and that therefore the distinction must not be carried too far.

REFERENCES

- (1) LANDIS, C. & ROSS, J. W. H. (1933). "Humor and its relation to other personality traits." *J. Soc. Psychol.* iv, 156-75.
- (2) HEIM, A. (1936). "An experiment on humour." *Brit. J. Psychol.* xxvii, 148-61.
- (3) STUMP, N. F. (1939). "Sense of humour and its relationship to personality, scholastic aptitude, emotional maturity, height, and weight." *J. gen. Psychol.* xx, 25-32.
- (4) EASTMAN, M. (1921). *Sense of Humor*. New York: C. Scribner's Sons.
- (5) EYSENCK, H. J. (1940). "The general factor in aesthetic judgements." *Brit. J. Psychol.* xxxi, 94-102.
- (6) GHOSH, R. (1939). "An experimental study of humour." *Brit. J. educ. Psychol.* ix, 98-9.
- (7) KAMBOUROPOULOU, P. (1926). "Individual differences in the sense of humor, and their relation to temperamental differences." *Amer. J. Psychol.* xxxvii, 268-77.
- (8) GUILFORD, J. P. & SPENCE, W. (1933). "Affective value of combinations of colors." *Amer. J. Psychol.* xlv, 495-501.
- (9) EYSENCK, H. J. (1941). "Psychological aspects of colour measurement." *Nature, Lond.*, cxlvii, 682-3.
- (10) MARTIN, L. J. (1905). "Psychology of esthetics; experimental prospecting in the field of the comic." *Amer. J. Psychol.* xvi, 35-118.

- (11) HOLLINGWORTH, H. L. (1911). "Experimental studies in judgment: Judgment of the comic." *Psychol. Rev.* xviii, 132-56.
- (12) EYSENCK, H. J. (1940). "Some factors in the appreciation of poetry, and their relation to temperamental qualities." *Char. & Pers.* ix, 160-7.
- (13) — (1941). "The empirical determination of an aesthetic formula." *Psychol. Rev.* xlviii, 83-92.
- (14) — (1939). "The validity of judgments as a function of the number of judges." *J. exp. Psychol.* xxv, 650-4.
- (15) GUILFORD, J. P. & GUILFORD, R. B. (1936). "Personality factors *S*, *E*, and *M*, and their measurement." *J. Psychol.* ii, 109-27.
- (16) BERGSON, H. (1911). *Le Rire, Essai sur la Signification du Comique*. Paris: Librairie F. Alcan.
- (17) CHANDLER, K. A. (1902). "The sense of humor in children." *Cent. Mag.* pp. 959-60.
- (18) KIMMINS, C. W. (1932). *The Springs of Laughter*. London: Methuen.
- (19) LUDOVICI, A. M. (1932). *The Secret of Laughter*. London: Constable and Co.
- (20) WRENCH, G. T. (1908). *Grammar of Life*. London: Heinemann.
- (21) MAIER, N. R. F. (1932). "A gestalt theory of humour." *Brit. J. Psychol.* xxiii, 69-79.
- (22) WILLMANN, J. M. (1940). "An analysis of humor and laughter." *Amer. J. Psychol.* liii, 70-85.
- (23) GREIG, J. Y. T. (1923). *The Psychology of Laughter and Comedy*. London: Allen and Unwin.
- (24) GREGORY, J. C. (1924). *The Nature of Laughter*. London: Kegan Paul.

(Note. No detailed references are given for the classical writers whose names are quoted in this paper. Full references will be found in the works of Eastman(4), Greig(23), and Gregory(24), together with brief but usually adequate summaries of their views.)

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FOLLOWING UP INDIVIDUAL ITEMS IN A GROUP INTELLIGENCE TEST

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- I. *Object of the enquiry* (p. 310).
- II. *The technique of item analysis* (pp. 311-312).
- III. *The data* (pp. 312-314).
- IV. *Discrimination between secondary school and intermediate school* (p. 315).
- V. *Discrimination within the secondary school* (p. 316).
- VI. *Discrimination within the intermediate school* (pp. 316-317).
- VII. *Conclusion* (p. 317).

I. OBJECT OF THE ENQUIRY

THERE have been many researches in which the predictions of an entrance group intelligence test, taken as a whole, have been followed up during the careers of children in secondary schools or in junior secondary schools,¹ more often in the former than in the latter. Other researches have compared the predictive power of different sections of the group test, as for example the *analogies*, the *number series*, the *absurdities*, and have found regression weights for combining these to give the best correlation with future success. But there have not, as far as I know, been any enquiries (and if any, there certainly have not been many) which have followed up each of the individual items of the test. This is what is done in the present article. The object of course is to gain still more information with a view to producing still better tests. At the back of my mind was also the suspicion, which has been somewhat deepened by the results of this experiment, that it is not so much the type of the item which matters (as *analogy*, or *absurdity*) as the difficulty value of the individual item. The research also throws light on the fact that the predictive power of a test *within* the secondary school is not necessarily, indeed not usually, the best indication of its power to separate the potential secondary school pupils from those not suitable for (academic) secondary school courses.

¹ Intermediate Schools, Central Schools, Advanced Divisions, are among the names given to these schools.

II. THE TECHNIQUE OF ITEM ANALYSIS

The technique to be used was already fully available, for it is the same as that used in selecting items to make a new test. To make a new test of 100 items we in Moray House devise 300 items, many of them similar to those of former tests, and give them (in suitable doses) to 300 children. These children are divided, by some means or other, into six equal groups, ranging from the cleverest to the dullest. Sometimes this can be done by asking the opinions of their elementary school teachers; sometimes by elementary school examination results; sometimes by an already established and trusted group intelligence test; most easily by the total results of the 300 new items themselves.¹

For each of the 300 items it is then ascertained how many children in each of these separate groups have answered it correctly; and broadly speaking, those 100 items are retained which discriminate best between the clever and the dull. But, as will appear below, much depends on whether we want to discriminate between the very clever and the nearly-as-clever, or between the mediocre and the dull, or at some other point, or generally over the whole range.

Various methods of calculating coefficients of discrimination have been published, mostly in the American journals, and we employ some of these in our laboratory. But for the explanatory purposes of this article it is better I think merely to draw graphs and compare them by eye. For example, in Fig. 1 (p. 314) are shown the graphs for two items. The dots represent the six groups of children from clever to dull, and the left-hand scale represents the proportion answering an item correctly. It is clear that item 1 is easy. None of the best two groups of children failed to answer it. Only the poorest group failed in any substantial numbers. This item asked "What is the fifth letter of the alphabet?" Despite its simplicity and lack of discrimination except at the lowest level of ability, it was retained in the test as its first item as a 'warming-up' item intended to give confidence, and indeed really to act as part of the instructions. Item 24, on the other hand, as is evident at a glance, is much more difficult, and discriminates over nearly the whole range, especially in the middle portion where the slope of its graph is steepest. This item asked for the next number after 1, 2, 4, 8, 16.

¹ This last plan suffers from the obvious disadvantage of being to some extent an argument in a circle, and I have for long desired to have some idea of how item selection by this means compares with actual predictive power, a question to which the present research, in conjunction with other data, gives an answer showing that this method is in fact not a bad one. But this point is not dealt with in the present paper.

312 *Following up items in a group intelligence test*

It is clear that this technique can be immediately used in a follow-up. The plan is to have the children divided into groups according to the opinion of the post-primary schools to which they have gone (after they had been in those schools for one year, in our case) and draw such graphs for each item.

III. THE DATA

The test used was Moray House Test 24, and the children were 752 Durham County children of whom 259 had been in four secondary schools, and 493 had been in eight intermediate schools, for one year.

Moray House Test 24, after being made, in 1935, in the manner described in § II, was used privately by thirteen Education Authorities in various parts of England and Scotland, ten of whom administered it to a *complete* age group, as shown in Table 1. The test was then published

Table 1

Code designation of area in Moray House records	No. of children	Age range
A	6,805	10-8 to 11-7
Y	4,476	10-8 „ 11-7
AE	3,430	10-2 „ 11-1
W	4,316	10-4 „ 11-3
D	818	10-0 „ 10-11
G	1,624	10-6 „ 11-5
AT	3,448	9-8 „ 10-7
AZ	1,872	10-5 „ 11-4
Q	1,006	10-6 „ 11-5
AF	2,227	10-8 „ 11-7
Total	30,022	

(by the University of London Press, Ltd.) with a conversion table, based on these 30,022 cases, for turning raw scores into 'intelligence quotients', with centre at 100 and standard deviation of 15 points. In this form it has since been used in other areas, among them Durham County, where it was in 1940 given to 11,462 elementary school pupils aged 10-8 to 11-7 for the purpose of selecting those who might enter secondary schools or intermediate schools. The mean intelligence quotient, calculated by the above-mentioned conversion table, was in County Durham 102.5 for the boys and 102.3 for the girls. The excess above 100 is explained by the fact that the group of 11,462 tested was not the *complete* age group of 13,923. The 2461 who did not take the test were partly absentees,¹ but included a large number of very dull children who were not given the test because they were considered in any case too dull for an intermediate school. The same absence of the very dull probably also explains

¹ About 557 of the 2461, presumably children absent through illness, were later tested by another test.

the standard deviations in Durham County, of 14.4 for boys and 13.4 for girls (instead of 15 for a complete age group). Of the whole year group, 1458 then entered secondary schools and the rest entered either intermediate schools or senior departments. Four of the secondary schools (with 59, 62, 82 and 56 entrants respectively) and eight of the other schools (with 55, 42, 68, 50, 30, 40, 66 and 142 entrants respectively) were chosen for the follow-up of individual items of the test. Six of these eight schools were called 'intermediate', the other two (one for boys and one for girls) were not so designated. For simplicity, however, the group is in what follows called the 'intermediate' group. After the children had been for one year in these twelve schools the headmaster (or headmistress) divided those in his school into three groups, as nearly equal as possible, according to their suitability for the work of that school. The 259 secondary pupils were thus divided into three groups of 86, 87 and 86, called groups *a*, *b* and *c* in what follows, while the 493 intermediate pupils were divided into three groups of 164, 165 and 164, called *d*, *e* and *f*. Of course it must be remembered that the separation of the three groups *abc* from the three groups *def* was actually made in 1940 by the total test score. The separation of *a* from *b*, and *b* from *c*, however, and similarly of *d*, *e* and *f*, was done a year later by the teachers, regardless of the test score. The head teachers of the secondary schools (Miss Hilda Cowie, Messrs Wm. Carr, J. C. Cleminson, and T. Oates) then undertook the labour of ascertaining, separately for each of these six groups *a*, *b*, *c*, *d*, *e* and *f*, how many times each of the 100 items had been answered correctly. Sincere thanks are due to all the twelve head-teachers, and especially to these four, for their co-operation; and also to the Director of Education, Mr T. B. Tilley, for his interest and assistance. A diagram was then made for each item showing what proportion of each of the six categories had succeeded with the item (see, for example, Fig. 2).

The group of 259 secondary pupils were, it may be presumed, a fair sample of the whole 1458 who entered secondary schools throughout the county. The group of 493, however, who form the intermediate school sample are not a random sample of the unsuccessful, but a somewhat superior sample of them. The intelligence quotients corresponding to the average raw scores of the six groups were: *a*, 129; *b*, 126; *c*, 120; *d*, 111; *e*, 103; and *f*, 93; for *abc* together, 125; for *def* together, 102. The average i.q. of all the children unsuccessful in entering secondary schools would be something below 100. Probably our 493 cases contain few representatives of the bottom twenty per cent (intellectually) of the children of the county. One of the intermediate schools on our list is 'selective'. The others are 'non-selective', but there has clearly been a certain amount of selection exerted not by authority but by circumstances.

Fig. 1

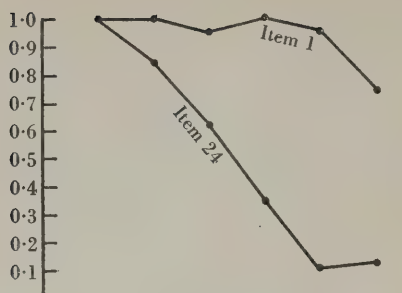


Fig. 2

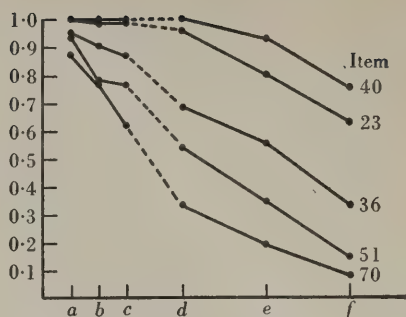


Fig. 3 (Item 68)

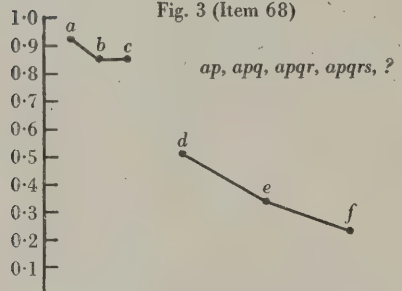


Fig. 4 (Item 70)

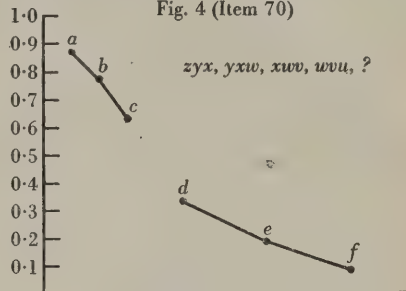


Fig. 5 (Item 90)

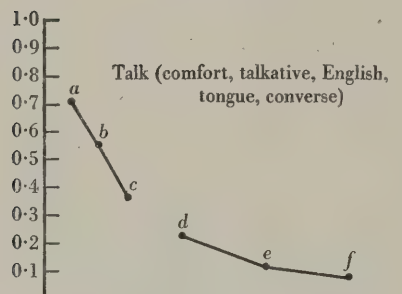


Fig. 6 (Item 60)

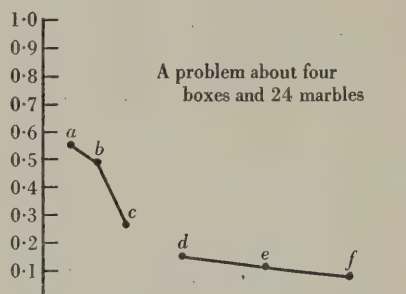


Fig. 7 (Item 28)

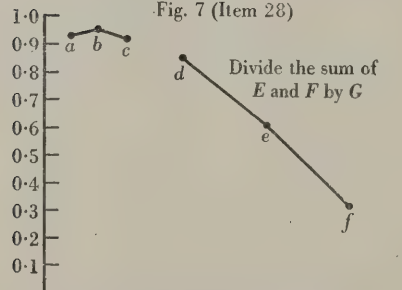
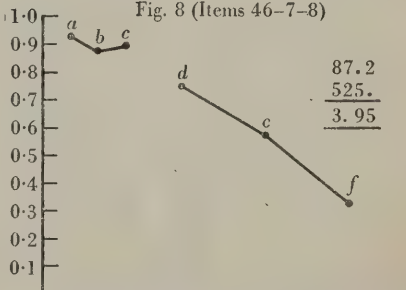


Fig. 8 (Items 46-7-8)



In these diagrams the points *a*, *b* and *c* refer to 259 secondary school pupils classified by their teachers after one year in the school, and *d*, *e* and *f* refer similarly to 493 intermediate school pupils. The ordinate of each point gives the proportion of that group who were successful in the item to which the diagram refers, in an intelligence test given before entrance to the school. The abscissae are not metrical, though the points are very roughly spaced, horizontally, as they might be expected to lie. Fig. 1 refers to a different group of children, a complete age group divided into six equal parts.

IV. DISCRIMINATION BETWEEN SECONDARY SCHOOL
AND INTERMEDIATE SCHOOL

The hundred diagrams thus made for the separate items show at a glance not only that the items are of different degrees of efficiency, but that they perform different functions (see Fig. 2). The most important function of the test was, I think, to separate from the rest those pupils who were suitable for an academic secondary school education. Those items were presumably most successful in doing this which on the diagrams show the widest gap between the point *c* (the bottom third of the secondary school a year later) and the point *d* (the top third of the intermediate school, also a year later). The widest gap of this sort was shown by item 68 (see Fig. 3) which asked for the next group of letters in the series ap, apq, apqr, apqrs; where the gap is 0.349. But this may partly be due to the somewhat anomalous position of the point *c*. The second widest gap between *c* and *d* was in item 70 (see Fig. 4) which asked for the next member of the series zyx, yxw, xwv, wvu. Among the twelve items in which the gap between *c* and *d* exceeded 0.200 (see Table 2) there were four of this type, and two others also concerned with groups of letters, in which the child was asked to underline (italicized in table) the 'different' group which should not be there.

Table 2. *Items discriminating best between the bottom of the secondary school and the top of the intermediate school*

Serial no.	Short clue to nature of item*	Point <i>c</i>	Point <i>d</i>	Difference
6	Different: 5, 3, 8, 28, 2	0.814	0.591	0.223
9	Different: 545, 767, 878, 313, 656	0.837	0.585	0.252
51	conceal (obey, unseen, hide, comfort, find)	0.768	0.543	0.225
59	How many marbles in box <i>C</i> ?	0.616	0.384	0.232
67	ll, mm, nn, oo, pp, next?	0.930	0.671	0.259
68	ap, apq, apqr, apqrs, next?	0.849	0.500	0.349
69	abc, bed, cde, def, efg, next?	0.733	0.463	0.270
70	zyx, yxw, xwv, wvu, next?	0.616	0.335	0.281
71	up is to down as climb is to ?	0.779	0.512	0.267
79	Different: ponm, <i>decb</i> , dcba, xwvu, kjih	0.419	0.177	0.242
80	Different: ceg, moq, ikm, <i>efh</i> , tvx	0.477	0.220	0.257
96	Rings and knocks on different days	0.674	0.445	0.229

* The actual items were longer and preceded by explanation.

The reader will not have failed to note that the absolute height of the points *c* and *d*, and not merely their difference, is of importance. For example, items 9 and 79 have about the same gap between *c* and *d*: but item 9 would admit many, item 79 only few, to the secondary school. Item 79 would keep out many deserving candidates, item 9 would admit many undeserving.

V. DISCRIMINATION WITHIN THE SECONDARY SCHOOL

The items which discriminate best between the bottom of the secondary school and the top of the intermediate school are not usually the same as those which discriminate best *within* the secondary school, a fact which should give pause to those who judge an entrance test by the concordance between final order of merit in the secondary school and order of merit in the entrance test. The best discriminator within the secondary school, judged by the difference between the point *a* and the point *c*, was item 90 (see Fig. 5) which asked which of the words *comfort*, *talkative*, *English*, *tongue*, *converse* means nearly the same as the word *talk*. Item 60, the next best (see Fig. 6) says: "I have 24 marbles and four boxes. Leaving box *D* empty, I place twice as many in box *B* as in box *A*, and twice as many in box *C* as in box *B*, and have 3 left over. How many marbles are there in box *A*?"

Table 3. *Items discriminating best within the secondary school*

Serial no.	Short clue to nature of item*	Point <i>a</i>	Point <i>c</i>	Difference
5	Long shadows from N.W., what time?	0.547	0.279	0.268
30	Ear is to nose as hear is to ?	0.768	0.512	0.256
31	Penny is to sixpence as shilling is to ?	0.779	0.558	0.221
58	How many marbles in box <i>A</i> ?	0.768	0.535	0.233
60	How many marbles in box <i>A</i> ?	0.547	0.267	0.280
70	zyx, yxw, xwv, wvu, next?	0.872	0.616	0.256
75	Two letters of AMBLE same distance apart in alphabet	0.663	0.454	0.209
76	Two letters of GOLD same distance apart in alphabet	0.768	0.512	0.256
80	Different: ceg, moq, ikm, <i>efh</i> , tvx	0.698	0.477	0.221
89	Rest (repose, activity, oppress, restive, bed)	0.744	0.465	0.279
90	talk (comfort, talkative, English, tongue, converse)	0.709	0.360	0.349
93	Two sentences (of four) meaning nearly the same	0.919	0.709	0.210
94	Two sentences (of four) meaning nearly the same	0.605	0.326	0.279
95	Problem about rings and knocks on door	0.465	0.244	0.221
99	Laundry put into alphabetical order	0.837	0.581	0.256

* The actual items were longer and preceded by explanation.

The fifteen items of Table 3 are, with two exceptions, not included in Table 2.

VI. DISCRIMINATION WITHIN THE INTERMEDIATE SCHOOL

Similarly the ten items of Table 4 are different entirely from the items of Tables 2 and 3. Discrimination within the intermediate school is best performed by items other than those which differentiate at the other levels. Fig. 7 shows the curve for the item 28, and Fig. 8 for the average of the three items 46, 47 and 48. The 'differences' in this Table 4 between points *d* and *f* are larger than those in the former tables because

there are more children in each group, and such selection as there is for the intermediate school is so much less stringent.

Table 4. *Items discriminating best within the intermediate school*

Serial no.	Short clue to nature of item*	Point <i>d</i>	Point <i>f</i>	Difference
8	Different: 55, 22, 45, 88, 66	0.671	0.250	0.421
24	1, 2, 4, 8, 16, next ?	0.774	0.317	0.457
27	Divide D by G (number code)	0.780	0.360	0.420
28	Divide sum of E and F by G (number code)	0.854	0.311	0.543
29	What from F to get G? (number code)	0.799	0.348	0.451
(46)†	From 87.2	(0.616)	(0.232)	(0.384)
47	Take 525.	0.884	0.427	0.457
48	3.95	0.738	0.280	0.458
50	Hasten (chase, stop, hurry, has, Hastings)	0.890	0.482	0.408
62	Wear girls hats some brown	0.933	0.530	0.403
87	Latest letter (alphabetic) in <i>criterion</i>	0.732	0.329	0.403

* The actual items were longer and preceded by explanation.

† Item 46 is only included because of its close association with 47 and 48.

VII. CONCLUSION

It is hoped that the publication of this article, with its very simple if somewhat laborious technique, will encourage others to follow up individual items of tests of intelligence (and of other kinds of tests too). The labour is not great if team work can be made use of, and the method is therefore very suitable for co-operative research by the members of a teachers' association. The paper may also serve to increase confidence in tests by illustrating the care taken in their construction and later checking, and to throw light on the fact (not easily appreciated by non-mathematicians) that discrimination within a highly selected group may be small even though (or indeed just because) the instrument of selection has been efficient. Prediction correlations within a less highly selected body (such as, for example, the children entering a Scottish secondary school) are always larger than those within a more highly selected body (as, in comparison, the smaller entry to an English secondary school is).

(*Manuscript received 18 November 1941*)

PARTIALLING OUT SUMS OF SQUARES AND PRODUCTS IN CALCULATING CORRELATIONS WITH NON-HOMOGENEOUS DATA

BY WILLIAM STEPHENS TAYLOR

- I. *The importance of partialling out sums of squares and sums of products* (pp. 318–320).
- II. *An example of the method* (pp. 320–322).
- References* (p. 322).
- Note by a referee* (p. 323).

I. THE IMPORTANCE OF PARTIALLING OUT SUMS OF SQUARES AND SUMS OF PRODUCTS

IN analysing psychological data it has been the normal practice to calculate correlations without attempting to partial out sums of squares and sums of products of deviations from the mean. Unless, however, one has independent evidence that the population tested is homogeneous, the values obtained may be misleading. They may consequently provide an unsatisfactory basis on which to build up factorial analyses.

In test results, or other quantitative psychological data, where the individuals tested fall into groups, the sums of squares of deviations about the mean are divisible into two parts—the sums of squares of the deviations of the group means about the grand mean, and the sums of squares of the deviations of the individual scores about their group means. In Fisher's⁽¹⁾ notation

$$S(y - \bar{y})^2 = S n_p (\bar{y}_p - \bar{y})^2 - SS(y - \bar{y}_p)^2,$$

where $\bar{y}$ is the mean of all the values of y , the suffix p denotes any particular group, n_p the number in the group, and $\bar{y}_p$ the mean of the group. Sums of products of deviations about the mean can also be divided into the same two parts in the same way. With appropriate modifications the same formula can be used of them.

Where the population tested is homogeneous, the differences between the mean values of the various groups are insignificant. The sums of squares and sums of products of the deviations of group means about the grand mean will then be entirely negligible, and a coefficient of correlation, calculated in the ordinary way directly from the deviations

of individual values about the grand mean, will give a reliable indication of the correlation between the scores of the individuals tested. Where the population is not homogeneous, however, group differences may be significant. Under these conditions, the value obtained by calculating the correlation coefficient in the ordinary way will be due to the averaging of an inter-class correlation between the means of the various groups and an intra-class correlation between the scores of the individuals composing the groups. The latter is the correlation desired, but its true value has been obscured by the inclusion of a value for the correlation of means. In order to obtain a true value for the correlation between the scores of the individuals composing the groups it is necessary to partial out the sums of squares (2) and sums of products of deviations from the mean, using only those attributable to the deviations 'within groups', i.e. to the deviations of the observations about their group means. The true value for the correlation between the scores of individuals, obtained after the sums of squares and sums of products have been partialled out in this way, may sometimes differ considerably from the values obtained in the ordinary way.

The following table of correlations (Table 1) illustrates this difference. In the upper half of the table are the correlation coefficients obtained

Table 1

Test no.	Test no.								
	1	2	3	4	5	6	7	8	9
1		*0.3611	*0.3966	*0.3761	0.1840	0.0422	†0.2876	0.1296	*0.3800
2	0.0691		*0.4014	0.2025	0.2061	-0.0084	0.1994	0.1065	0.2239
3	†0.3264	*0.3703		*0.5161	†0.2850	-0.0503	0.0361	0.0995	†0.3326
4	0.2670	0.0492	*0.4294		†0.2966	-0.1479	-0.0120	-0.1962	*0.3780
5	0.2158	0.2071	†0.2865	†0.3098		0.0059	†0.3001	0.0318	*0.3589
6	0.2492	0.2313	0.0758	-0.0619	0.2190		†0.3374	†0.4333	-0.1002
7	*0.3686	0.2041	0.0465	-0.0071	0.2373	*0.5107		0.2003	0.1484
8	0.1360	0.1385	0.1384	-0.1992	0.0960	*0.4036	0.2505		-0.0949
9	0.1700	-0.0488	0.2524	0.2022	*0.3637	0.1212	0.1601	-0.0796	

* Significant on the 0.01 level.

† Significant on the 0.05 level.

The tests used, and the numbers by which they are referred to, are as follows:

- | | | |
|-----------------------------|--------------------------|---------------------------|
| (1) Jumbled sentences test. | (4) Absurdities. | (7) Cancellation P R S T. |
| (2) Number series. | (5) Sentence completion. | (8) Retention. |
| (3) Analogies. | (6) Cancellation A. | (9) Logical memory. |

in the usual way by calculating the deviations of all observations about the grand mean. In the lower half are the correlations obtained after partialling out sums of squares and sums of products. The coefficients in both parts of the table were obtained from the scores, on a battery of nine tests, of three successive classes of students in an Indian college. The students were divisible into two groups according to their native

language. In calculating the second set of coefficients, the sums of squares and sums of products for class differences, language differences, and their interactions were partialled out.

In many cases the corrected coefficients are considerably different from those obtained in the usual way: e.g. r_{12} , r_{14} , r_{16} , r_{17} , r_{19} , r_{24} , r_{26} , r_{29} , r_{34} , r_{49} , r_{56} , r_{67} . In cases such as these the uncorrected coefficients would be misleading.

There are a great variety of differences, of age, education, economic standing, language, social background, etc., which might introduce confusing group differences into an analysis of the scores of any given population. Under the best conditions, the meaning of the correlation coefficient is ambiguous. When the intra-class correlation which is desired is complicated by the admixture of inter-class correlations between means of groups it becomes even more ambiguous, and as a statistic may be definitely misleading. Unless one has independent evidence that the population tested is homogeneous, it is therefore important to partial out sums of squares and sums of products attributable to group differences before calculating the correlation coefficient.

II. AN EXAMPLE OF THE METHOD

The following hypothetical example¹ will serve to illustrate the method by which sums of squares and sums of products may be partialled out. A hypothetical example, with exactly ten students in each group, is given in Table 2 to simplify the calculations.

Partialling out sums of squares of deviations from the mean for test I

Total s.s. of deviations from the mean

$$= S(x - \bar{x})^2 = (Sx)^2 - \frac{S(x^2)}{N} = 28746 - \frac{994^2}{40} = 28746 - 24700.9 = 4045.1.$$

Total s.s. of deviations attributable to language differences, class differences, and interactions

$$= \frac{(Sx_1)^2 + (Sx_2)^2 + (Sx_3)^2 + (Sx_4)^2}{\text{No. of students per group}} = \text{c.f.*} = \frac{293^2 + 183^2 + 322^2 + 196^2}{10} - \text{c.f.}$$

$$= 26143.8 - 24700.9 = 1442.9.$$

Total s.s. of deviations within groups is the difference between the above two values, or 2602.2.

¹ A purely hypothetical example has been given, with the same number in each group, as in the actual material used the numbers in the groups were unequal, and this slightly complicated the calculations. An example of the calculation for groups of unequal numbers will be found in (2), section 69.

* c.f. (correction factor) is for sums of squares always the quantity $\frac{(Sx)^2}{N}$, and for sums of products the quantity $\frac{Sx.Sy}{\text{No. of pairs}}$.

Table 2

	Test I		Test II		xy
	x	x^2	y	y^2	
1938 class:	40	1600	20	400	800
Hindi	30	900	22	484	660
speaking	35	1225	18	324	630
	20	400	15	225	300
	38	1444	14	196	532
	42	1764	30	900	1260
	32	1024	32	1024	1024
	24	576	20	400	480
	20	400	18	324	360
	12	144	8	64	96
	293	9477	197	4341	6142
Non-Hindi	25	625	30	900	750
speaking	20	400	35	1225	700
	15	225	40	1600	600
	28	784	20	400	560
	30	900	22	484	660
	10	100	25	625	250
	15	225	15	225	225
	20	400	28	784	560
	12	144	24	576	288
	8	64	35	1225	280
	183	3867	274	8044	4873
1939 class:	35	1225	20	400	700
Hindi	30	900	25	625	750
speaking	40	1600	15	225	600
	42	1764	10	100	420
	45	2025	20	400	900
	40	1600	18	324	720
	25	625	25	625	625
	30	900	22	484	660
	20	400	24	576	480
	15	225	25	625	375
	322	11264	204	4384	6230
Non-Hindi	12	144	20	400	240
speaking	20	400	18	324	360
	18	324	18	324	324
	15	225	25	625	375
	25	625	20	400	500
	20	400	25	625	500
	24	576	18	324	432
	30	900	15	225	450
	20	400	12	144	240
	12	144	20	400	240
	196	4138	191	3791	3661
Grand total	994	28746	866	20560	20906

Partialling out sums of squares of deviations from the mean for test II

Using the same method as for test I, we get the following values:

Total s.s. of deviations from the mean

$$= 20560 - 18748 \cdot 9 = 1811 \cdot 1.$$

Total s.s. of deviations attributable to group differences, etc.

$$= 19198 \cdot 2 - 18748 \cdot 9 = 449 \cdot 3.$$

Total s.s. of deviations within groups = 1361·8.

Partialling out sums of products of deviations from the means for both tests

Total s.p. of deviations from the means

$$= S(x - \bar{x}) \cdot S(y - \bar{y}) = S(xy) - \frac{Sx \cdot Sy}{\text{No. of pairs}} = 20906 - \frac{994 \cdot 866}{40} = -614 \cdot 1.$$

Total s.p. of deviations attributable to group differences and interactions

$$\begin{aligned} &= \frac{(Sx_1 \cdot Sy_1) + (Sx_2 \cdot Sy_2) + (Sx_3 \cdot Sy_3) + (Sx_4 \cdot Sy_4)}{\text{No. of pairs of students per group}} - \text{c.f.} \\ &= \frac{(293 \cdot 197) + (183 \cdot 274) + (322 \cdot 204) + (196 \cdot 191)}{10} - 21520 \cdot 1 = -421 \cdot 4. \end{aligned}$$

Total s.p. of deviations within groups = -192·7.

Calculating the Pearson product moment correlation for the two tests using total values before s.s. and s.p. for group differences, etc. have been partialled out

$$r_{12} = \frac{Sd_1 d_2}{\sqrt{\{S(d_1^2) \cdot S(d_2^2)\}}} = \frac{-614 \cdot 1}{\sqrt{(4045 \cdot 1 \cdot 1811 \cdot 1)}} = -0 \cdot 2269.$$

Calculating the Pearson product moment correlation for the two tests after the s.s. and s.p. for group differences, etc. have been partialled out

$$r_{12} = \frac{Sd_1 d_2}{\sqrt{\{S(d_1^2) \cdot S(d_2^2)\}}} = \frac{-192 \cdot 7}{\sqrt{(2602 \cdot 2 \cdot 1361 \cdot 8)}} = -0 \cdot 1024.$$

REFERENCES

- (1) FISHER, R. A. (1938). *Statistical Methods for Research Workers*. Edinburgh: Oliver and Boyd.
- (2) SNEDECOR, G. W. (1934). *Calculation and Interpretation of Analysis of Variance and Covariance*. Iowa: Ames.

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NOTE BY A REFEREE

The device explained in Prof. Taylor's short paper, which has been in use in at any rate one educational psychology laboratory for some years, can be perhaps more concisely expressed thus. When the individuals tested fall into groups, and the correlation desired is that free from the influence of group differences, it is best to calculate the sums of squares and of products *separately for each group from its own means* (as would naturally be done as the experiment proceeded) and then simply to add these.

Thus in his own example of four groups of ten students each, the calculation on these lines would appear as follows, where 892.1 is $9477 - 293^2/10$, 369.9 is $6142 - 293 \times 197/10$, and so on:

	Σx^2	Σxy	Σy^2
Hindi 1938	892.1	369.9	460.1
Non-Hindi 1938	518.1	- 141.2	536.4
Hindi 1939	895.6	- 338.8	222.4
Non-Hindi 1939	296.4	- 82.6	142.9
Totals	2602.2	- 192.7	1361.8

$$r = -192.7/\sqrt{(2602.2 \times 1361.8)} = -0.1024.$$

CRITICAL NOTICE

By ROBERT H. THOULESS

The Factors of the Mind: an Introduction to Factor-analysis in Psychology.

By CYRIL BURT. London: University of London Press. 1940.

Pp. 509. 21s. net.

It is a fortunate circumstance for the development of psychology in this country that the outbreak of war led Prof. Burt to put into the form of a book the fruits of his unique knowledge of factor analysis which have been previously only fully available to the students of University College, London, and in a scattered and incomplete form for other psychologists in his articles on the subject in the technical journals. While there are now several books on the subject of factor analysis, it has been difficult for the student of psychology to gain a satisfactory understanding of the subject, since most of these have been written primarily in defence of the methods of their authors, and their tendency to deny any 'psychological meaning' to rival methods may well have led the not very sympathetic enquirer to doubt whether any of the methods had sufficient psychological meaning to be worth studying. Burt has his own contributions to make to method, but gives a sympathetic and understanding account of other methods which he regards as different but not necessarily erroneous ways of achieving what is virtually the same result. So his book succeeds in giving the student not merely an introduction to a particular method of factor analysis but an understanding of the whole subject.

The point of factor analysis may be expressed as an attempt to describe the abilities of any individual as so much intelligence, so much verbal ability, and so on. In other words, to specify an individual's abilities as $Ag + Bv + \dots$, where g , v , etc. stand for abilities and A , B , etc. are numbers showing the amounts of each different ability possessed by the individual. Obviously, the same method may also be applied to other mental measurables than ability, and it may be equally possible to express his temperament as $Ma + Np + \dots$. Also the analysis provides a method of specifying the abilities required in any particular test as $A'g + B'v + \dots$, where A' , B' , etc. are test saturations with the abilities in question. One problem of factor analysis is, what are the factors, g , v , etc., that we ought to use for this purpose? Another is to devise methods by which amounts and test saturations of these factors can be estimated.

As a branch of psychological study, factor analysis has suffered from the opinion that it is an isolated branch of psychological research little related to the main current of psychological investigation and of such mathematical difficulty that it can only interest those with exceptional mathematical ability. It must be admitted that the quantitative determination of factors does require some knowledge of appropriate mathematical methods, although the difficulty of acquiring this knowledge is much less than is commonly supposed. The general principles of thought about factors, on the other hand, are, as Burt points out, neither new nor unfamiliar. It is a way of thinking found very generally in psychology and was known a good deal earlier than the special

methods for quantitative estimation of factors. Burt gives two examples of earlier ways of thinking in terms of factors, although the term 'factor' was not used in them. The first is Wundt's tri-dimensional theory of feeling which was essentially an analysis of the complexity of feeling states into three orthogonal bi-polar factors. The second is the attempt of Helmholtz and other experimentalists to explain colour sensations as combinations of different amounts of a limited number of elementary or primary colours.

Perhaps the essential familiarity of the factor concept can be even better realized if we consider that it is found not only in psychology and related sciences but even in popular thinking. It is fundamental to popular political classification. We divide men into conservatives and radicals. This is a general bi-polar factor. We recognize that a man may be conservative or radical to any degree and also that we may say that a proposed measure is extremely conservative or radical, or somewhat so; in other words, we make the distinction between the amount of the factor possessed by an individual and the factor saturation of an Act of Parliament (which is here analogous to a mental test). We recognize, too, that the factorization into conservative and radical does not account for the whole of the political difference between men. We find some conservatives and some radicals who believe in establishing their ideals by violence (fascists and communists) and some of both parties opposed to violence. So more of men's political differences can be expressed by reference to this additional orthogonal factor, and if further exactness in the description of political differences were required, this could be attained by reference to other political factors. Furthermore, it would be generally recognized that the analysis of political differences might be made in other ways. The first fundamental distinction might be between individualists and socialists, a system of classification correlated with but not identical with the distinction between conservatives and radicals. From this starting-point, a different system of factors could account for the same political differences as those accounted for by the system of factors starting from the conservative-radical factor. This is exactly analogous to the different systems of factorization of abilities or other mental functions obtained by rotation of axes. There seems to be no sufficient reason for regarding the way of thinking in terms of factors as strange either to psychology or even to our more ordinary thinking. The novelty is rather in the application of precise quantitative methods to determine possible systems of factorization.

The early part of Burt's book is an admirable discussion of the purpose of factor analysis. It is possible that this would more appropriately have followed the description of methods, since it is only possible to understand the theoretical questions involved if the reader has already understood the practice and the results of factor analysis. The question of order is not, however, important, since it is to be hoped that the reader after finishing the book will return to the earlier chapters, which may then be read with increased insight.

The methods of factor analysis are a natural extension of Spearman's pioneer work on the general factor. Although modern workers in factor analysis have abandoned Spearman's original methods and at least some modern factorists reject his principal conclusion of the importance of the general factor, it remains true that his work is the foundation on which later work has been built, and the diversity of modern methods and even of modern conclusions shows how fruitful for psychology has been his original hypothesis.

Although Spearman was far from excluding the possibility of 'group

factors' common to groups of abilities, his primary problem was to demonstrate that most of the correlation between abilities could be accounted for by one general factor common to all abilities and specific factors peculiar to particular abilities or at any rate common only to very closely related abilities. In other words, the equation with which he was primarily concerned was the two-factor equation $T = Ag + Bs_T$, where g is the general factor and s_T is the specific factor for the particular test in question. For this purpose, he devised the method of determining all possible tetrad equations between tests and demonstrating that they did not differ significantly from zero.

Although laborious, this procedure involved no unfamiliar mathematical methods. When, however, correlations between larger numbers of subjects had shown that group factors could not be ignored and when large numbers of tests were used in a single research, it became necessary to use mathematical methods more economical in time than the calculation of innumerable tetrad differences. The required methods were found in matrix algebra.

These methods were not familiar to many psychologists, and they have not always been very lucidly explained by those psychologists who have used them. One of the demands, therefore, which may reasonably be made of Burt's book is that it should provide sufficient guidance as to the methods used for any student of psychology to be able to master them for himself. This requirement it fulfils very well. There are adequate examples fully worked out, and with so much knowledge of matrix algebra as can be obtained in a few hours' study of the more elementary parts of any text-book of the subject (such as A. C. Aitken's *Determinants and Matrices*), and by working out for himself some factor analyses under the guidance of Burt's book, any psychologist without any particular mathematical gifts should be able to understand the essential methods of factor analysis.

There is more than one possible view of what aim factor analysis sets out to achieve. When Spearman showed that, within the limits of error of his results, a matrix of correlation coefficients of dissimilar tests could be analysed into the effect of one general factor common to all the tests and a number of specific factors peculiar to each, the conclusion he drew was that this general factor was a unitary power of the mind. If later research with larger numbers of cases (and therefore smaller errors) had confirmed the fact that only one factor common to a number of dissimilar tests could be found, the evidence for this conclusion would have been overwhelmingly strong.

It is now, however, generally agreed that a matrix of correlation coefficients between tests cannot be explained completely by the two-factor equation, and that other factors common to many or all of the tests are to be found in addition to Spearman's general factor. Moreover, any matrix of correlation coefficients can be analysed into factors in an indefinitely large number of ways which are mutually derivable from one another by a process which we can represent geometrically as a rotation of axes or algebraically as a post-multiplication by an orthogonal matrix. By this process, a system of factorization showing a large general factor may be converted into one with equal mathematical validity in which there is no factor common to all the tests. It may well be that Spearman was right in believing that his general factor does correspond to a unitary mental capacity, but it does not seem any longer possible to maintain that this can be proved by any statistical analysis. This is also true of the factors revealed by a process of multiple factor analysis. If a real meaning can be attached to the statement that intelligence, verbal

capacity, etc. are unitary mental capacities, it remains true that this unitariness cannot be proved by factor analysis, and also that we cannot know that the proper figures for test saturations with these unitary capacities are those obtained by the use of one of the possible ways of analysing the original correlation matrix.

One way out of this difficulty is to find some ground for asserting that a particular method of analysis is the one right way which gives a real picture of the structure of mental abilities. Thus Thurstone believes that if, after a centroid analysis, the axes are rotated in such a way as to reduce to a minimum the number of negative saturations, and to increase as far as possible the number of zero saturations, factors will be obtained which correspond to real unitary mental capacities. This belief depends on the somewhat subjective criterion that the factors so obtained have more 'psychological meaning' than have other possible factor systems. Burt adopts the more radical alternative of abandoning the aim of distinguishing unitary mental capacities as a goal of factor analysis and regards his factors rather as analogous to the lines of latitude and longitude on a map. They "are to be thought of in the first instance as lines or terms of reference only, not as concrete psychological entities" (p. 18). This does not, however, mean that the factors are without scientific value or validity. "In order to give an adequate description of persons we must first discover in what *independent* directions a person may vary, and, at the same time, so far as possible, choose the direction so that each may carry with it a maximum amount of *dependent* variation" (p. 18). The last clause seems to be not quite exactly worded. The condition required for the greatest usefulness in description (and that actually secured by the method of weighted summation preferred by Burt) is not that each factor shall carry a maximum amount of dependent variation but that any small number of factors shall carry the maximum possible amount of dependent variation which can be carried without increasing the number of factors. This means that the maximum amount of variance is accounted for by the first factor, the maximum amount of the remaining variance by the second, and so on. This means that a general factor is retained in Burt's analysis and that it accounts for as much of the test results as can be explained by a general factor. This retention does not, however, necessarily imply that there is any psychological reality in the general factor, since it may be sufficiently justified as a measure of practical convenience.

The requirement that the greatest possible amount of the variance shall be accounted for by the smallest possible number of factors is obviously the most convenient condition to make for a purely descriptive analysis. It is, indeed, one that would commonly be recognized in everyday life. If we return to the consideration of the factor analysis of political differences, we see that two advantages may be claimed for making a first analysis into a liberal-conservative factor: (a) that it corresponds to a real social fact since there are actually liberal and conservative parties, and (b) that the differences between liberalism and conservatism may be considered to account for most of the political differences between men. A social theorist might write an article for a technical journal which began with the sentence: "There is a more fundamental difference between the political outlook of men than that expressed by the distinction between liberal and conservative." This new distinction might be (let us say) that between individualism and collectivism. In the language of factor analysis, he is saying that more of the variance in

political beliefs between men is accounted for if we rotate the axis of our first factor from the liberal-conservative to the individualist-collectivist position. It would generally be agreed that if the new first factor really accounted for more of the political differences between men, it might be more convenient to adopt this as the first method of distinguishing between men's politics even though this meant giving up the advantage of making our principle of discrimination coincide with the social reality of the actual direction of party organization.

It may seem to some that Burt makes a disappointingly modest claim for a technique which is often regarded as the one scientific method of finding out what are the unitary constituents of the mind. To this objection, the reply may be made that the usefulness of lines of latitude and longitude is not dependent on any correspondence they may have with real characteristics of the globe. Moreover, the problem of what are unitary constituents of the mind (if it is agreed that this is a real problem) cannot be solved independently of the results of factor analysis. If factor analysis cannot, by itself, tell us what are the unitary capacities of the mind, it can tell us what are and what are not possible ways of making an analysis of mental capacities.

The problem of alternative factor analyses can perhaps best be made clear by taking an artificial numerical example. Let us suppose that four tests were used (a smaller number, of course, than would be used in practice for factor analysis). These may be supposed to be a test *PG* of plane geometry, *AR* of arithmetic, *AL* of algebra, and *SG* of solid geometry. Suppose that the correlation coefficients between these tests were those of the matrix given below:

	<i>PG</i>	<i>AR</i>	<i>AL</i>	<i>SG</i>
<i>PG</i>	—	0.685	0.42	0.51
<i>AR</i>	0.685	—	0.52	0.385
<i>AL</i>	0.42	0.52	—	0.21
<i>SG</i>	0.51	0.385	0.21	—

Analysis by Burt's method, in which as much of the difference between testees as possible is accounted for by the first factor, gives the following matrix of factor saturations:

	I	II
<i>PG</i>	0.87	0.21
<i>AR</i>	0.83	-0.19
<i>AL</i>	0.56	-0.29
<i>SG</i>	0.52	0.26

This seems to show a general factor (I) common to all four tests but most strongly operative in plane geometry, and a factor (II) which is bi-polar (showing both positive and negative test saturations) and which shows the greatest influence on solid geometry and the maximum negative influence on algebra. A natural interpretation of this would be that it was the contrast between test performances dependent on skill in spatial apprehension and those dependent on manipulation of symbols.

The objection has, however, been made by Thurstone and others that negative saturations imply a capacity which makes a performance worse if testees possess that capacity and that they can therefore have no reasonable psychological meaning. To this, Burt makes two replies. First, that the objection could only be valid if it were admitted that a factor must measure a real psychological entity, but if we regard a factor as a line of reference, there

seems to be no reason why we should not choose a line of reference which marks a contrast between two abilities. Secondly, that apart from all quantitative factor analysis, we do use just such bi-polar terms in psychology. An obvious example is introversion-extraversion. Either can be treated as the positive pole, but any measurement positively saturated with one must be negatively saturated with the other.

If, however, we feel that a factor analysis can only have psychological meaning if there are no negative saturations, we can, in this particular case, very easily get rid of negative saturations by rotating the factor axes through 45° . This will then give a new set of factor saturations:

	I'	II'
PG	0.465	0.76
AR	0.72	0.46
AL	0.60	0.19
SG	0.18	0.555

Now we have two new factors both of which are, to some extent, common to all four tests but one of which (I') is especially characteristic of arithmetic and to a somewhat less extent of algebra, while the other is found most strongly in plane geometry and not quite so much in solid geometry. If anyone chose to make his factor analysis in this way, he would probably call I' a factor of numerical ability and II' a factor of spatial apprehension, and suggest that both factors entered to some extent into all four tested operations.

A rotation through 45° would, however, be somewhat arbitrary, since other rotations would equally well achieve the aim of getting rid of negative saturations. Let us suppose, therefore, that we wish to get the more rigidly defined solution of an analysis in which there are as many zero saturations as possible. In this particular case, we can only get zero saturations in two of the factors by increasing the number of factors above the minimum number of two and retaining also a general factor common to all tests. By appropriate rotations of the axes in three dimensions, we can get the analysis:

	I''	II''	III''
PG	0.76	0.44	0.17
AR	0.70	0.17	0.45
AL	0.46	0	0.43
SG	0.46	0.36	0

The psychological explanation is now likely to be that I'' is a factor of general ability, II'' a factor of ability to deal with spatial relations, and III'' a factor of ability to deal with symbols.

We now have three solutions of the original problem and many others might have been obtained. All are genuine solutions in the sense that if one had such factors in such saturations, the original matrix of correlations would have been obtained. This is shown by the fact that each matrix of saturation coefficients post-multiplied by its transpose gives (apart from its principal diagonal) the original matrix of correlation coefficients.

Close examination of these and other possible solutions shows, however, that they are less diverse than might appear at first sight. Their most essential features are common to all. In all, there is a tendency for arithmetic and algebra to resemble each other and for solid geometry and plane geometry to resemble each other. In the first analysis this tendency appears as the positive and negative saturations respectively of the two pairs of tests with

a single bi-polar factor; in the second and third analyses it is shown as a high saturation of the two pairs of tests with two separate factors. In all analyses the possibility of a general factor is shown; in the first and third this appears as a separate general factor, in the second as a positive saturation of all tests with each of the two factors.

There are certain features of the matrix of factor saturations which remain invariant through all rotations of axes. These invariant features may be regarded as genuine characteristics of the relationship between the tests, revealed in different ways by all methods of analysis. If we regard factors merely as axes of reference for the quantitative description of tests, it is reasonable to regard all different analyses obtained by different rotations as equally valid for this purpose, although all may not be equally convenient in practice. There may still be argument as to the equal validity of analyses obtained by different methods of filling up the missing principal diagonal of the original correlation matrix—by inserting reliability coefficients, or unity (Hotelling), or by successive approximation to the sum of the squares of the corresponding saturation coefficients (Burt), etc.

We can perhaps best think of a factor as the system of measurements given by an imaginary test battery. The possibility of alternative factor analyses of the same matrix of correlation coefficients means simply that there is more than one system of imaginary tests which could give a complete picture of the results of the actual test measurements from which the correlation matrix was constructed.

The question of what are the right factors to use in factor analysis arises, however, if we believe that there are such things as unitary mental capacities and that it is desirable to use factors which correspond as closely as possible with these unitary capacities. In any case it arises as a question of practical convenience if we want to make any comparison between the results of factor analysis of different samples of tests by different investigators or to make any sort of practical use of the factors distinguished.

A possible use of the results of factor analysis in test construction is to make batteries of tests which are saturated as highly as possible with the factors obtained by any one method of factor analysis. In other words, test batteries are constructed which correspond as closely as possible to the imaginary test batteries which are the factors. This has, of course, been done in the construction of tests saturated as highly as possible with g , with p , with v , etc. Unless, however, we have reasons outside the results of factor analysis for deciding what really are the unitary abilities which ought to be measured by tests, there must be some element of arbitrariness in the selection of what system of factors is to be chosen as the basis of measurement. It is obviously not completely arbitrary since, although alternative directions of measurement are allowed by the possibilities of alternative methods of factor analysis, a much larger number of directions of measurement are excluded since they correspond to no possible system of factor analysis.

One way of dealing with this element of arbitrariness is to admit its presence and to define the factors to be used for measurement as the abilities measured by certain batteries of tests. Thus we can make a battery of tests to measure g and define g as the capacity measured by that battery of tests. Similarly, we can define p , v , etc., bearing in mind always the limitation that the system of capacities so defined must be consistent with some system of factor analysis. It is not necessary to make any assertion of the independent

reality of the capacities so defined. This way of defining factors has been suggested recently. If adopted, it would be a reversion to the method adopted in the early days of factor analysis by W. P. Alexander in his monograph on *Intelligence, Concrete and Abstract*. Such a practice (which is equivalent to using a real test battery instead of an imaginary one to supply the lines of reference in a factor analysis) would be more economical of effort than the present method of every research worker making an analysis of the particular set of tests on which he is working into its own factors. The present method also has the disadvantage that the factors obtained depend on the particular sample of tests with which the work has been carried out; geography would be a very complicated subject if those who studied different parts of the earth's surface used different systems of lines of latitude and longitude.

An important feature of Burt's book is its adequate treatment of the problem of the statistical significance of factors. There is a considerable amount of published factor analysis in which this problem is disregarded or treated by quite inadequate methods, and factor saturations have been published when the numerical evidence for the existence of the factor in question has been very slender. It has probably often been the case that the later factors in published analyses have been characteristics only of residuals which are merely random collections of figures. Burt's interest is in scientific research and not the working out of mere arithmetical puzzles, and he provides adequate safeguards against this source of error. While, however, he employs means of testing whether there is statistically significant evidence for the existence of a further factor at any stage of the analysis, it is unfortunate that no means seems to be provided for estimating the limits of reliability of the saturation coefficients calculated for a factor which is proved to be really present. Thus when it is said that the saturation of a particular test with a particular factor is 0.372, one does not know whether this means that there is sufficient evidence that the value lies between 0.370 and 0.374 or whether it is only known to lie between 0.2 and 0.5. Perhaps factor analysts will supply this need in the near future.

The few defects which dim the lustre of this book are probably to be attributed to the haste in its preparation for the press which was necessitated by the outbreak of war. Some of the physical analogies in the early chapters seem to have been not very happily chosen. There are occasional patches of unnecessary obscurity, as, for example, the treatment of analysis of variance. Sometimes, there is a looseness of phrasing, as when, on p. 379 and elsewhere, the author speaks of the 'variance' of a trait. The mathematical term 'variance' can properly only be applied to a measurement, not to the thing measured, and it may be dangerously misleading to ignore this distinction in psychological measurements, since it leads to an overlooking of the arbitrary element in every psychological measurement.

There are many similar points with which a critic might quarrel, and perhaps one of the services of Burt's book when more normal times return will be to stimulate controversies which will elucidate many of the obscure points of factor theory. If there are defects, these are trivial; the merits of the book are great. It may safely be judged to be one of the most important contributions to psychology made during recent years.

PUBLICATIONS RECENTLY RECEIVED

The Second Yearbook of Research and Statistical Methodology. Edited by OSCAR KRISEN BUROS. Highland Park, New Jersey: The Gryphon Press. 1941. Pp. xx + 383. \$5.

This book is a second and much larger volume on the same lines as *Research and Statistical Methodology Books and Reviews*, 1933-1938, under the same editorial. It consists of a list of some 359 books on research and statistical methods published in 1933 or after. For each book is given a selection of extracts of critical reviews which have been published in various scientific and academic journals. The books reviewed are listed alphabetically under authors, and include a great variety of technical literature, comprising (to take a random sample) such subjects as Agriculture, Education and Psychology, Marketing, Plant Diseases, Vital Statistics and Zoology. The volume should be of considerable use to students in many fields of study who wish to know what reviewers think of books which they contemplate reading. Extracts from reviews appear to have been carefully and impartially made, so that adverse criticism as well as praise is recorded. Wherever known the names of reviewers are also given, which is in some cases a guide to the respect one may show some particular review. Another useful point about the volume is that the student may learn what books have been published recently in the particular branch of learning which is engaging his interest.

Should the volume be sufficiently well received and adequate financial aid be forthcoming, the editor proposes to make three major improvements in the next edition. He suggests including books written in foreign languages, abstracts of articles appearing in scientific periodicals and critical reviews made by "able and frank-speaking specialists" of scientific articles and papers not appearing in book form. These improvements, especially the last two, should increase the value of the publication greatly. It is particularly the case in statistical literature that new techniques are often described in journals which are not always readily available to students and there is frequently a considerable time lag before such methods are introduced into text books and become widely known. The inclusion of the essentials of such articles in one volume would greatly expedite the learning of new methods by a larger population of students and research workers.

The present volume is well printed on good paper and, once one has mastered the indexing system, is easy to use.

The Psychology of Conversation. By T. H. PEAR. Discussion Books No. 51. London: Thomas Nelson and Sons, Ltd. 1939. Pp. 171. 2s. 6d. net.

In the introduction to this book Prof. Pear says that "psychologists ought not to avoid problems of human behaviour because they are popularly interesting". This theme he has developed at some length in a more recent paper in this *Journal*. That psychologists do to some extent avoid direct treatment of problems such as that of 'conversation' (except between children) is in a sense true. But I think this may be due to the fact that many psychologists doubt their ability to advance their study of the subject *beyond* the stage of popular interest, where it may be left to the novelist and the essayist. If the psychologist is to tackle it usefully he must be prepared, first, to analyse it according to its underlying causes, and secondly, to systematize these causes in accordance with some general scheme of human behaviour and personality. Now apart from conversations, the main object of which is the exchange of ideas and information—and these are probably in the minority—con-

versation appears to have two main functions: (1) social: the attempt to establish a friendly social relationship, in which the *manner* (tone of voice, phraseology, etc.) is everything, the *matter* comparatively irrelevant; (2) ego-centric: the expression of the speaker's personality ('letting off steam'), often combined with the attempt to impress it upon others to a greater degree than they can impress their personalities upon him (this often occurs at committee meetings and scientific discussions). Clearly these, and any other, functions of conversation demand a differential approach and treatment. How they have developed in civilized society, and their relationships to the personalities of the conversationalists, provide a fascinating study. But it is likely to be successful only if it is guided by some systematic theoretical approach; just as some order has been effected among the chaos of dream material and neurotic phantasy primarily by means of the Freudian theory, even if the latter is more of a symbolic representation than a scientific statement of truth.

Prof. Pear has in this book produced most valuable Prolegomena to such a study, but the latter still awaits completion.

Heredity in Mental Traits. By N. N. SEN GUPTA. London: Macmillan and Co., Ltd. 1941. Pp. xii+207. 7s. 6d. net.

As a general survey of what is known on the inheritance of mental traits, this book should prove useful to the elementary student of the subject. It includes also a discussion of theories of heredity, and of the work of McDougall and others on the Lamarckian hypothesis. As a detailed study of exact scientific investigation of inheritance, it is less satisfactory. It was perhaps unavoidable that much of the data described should be anecdotal rather than fully substantiated, and the conclusions drawn from them necessarily negative in the main. But the work on inheritance of intelligence might have been treated in more detail, and references should have been given in full. There is, for instance, only a brief résumé of the work of Newman, Freeman and Holzinger on twins. Neither is there any allusion to that of Blatz on the Dionne quintuplets.

However, these shortcomings are perhaps inherent in the nature of the treatment accorded to the subject of heredity—the battleground for extreme environmentalists on the one hand, and of typologists and of uncritical believers in theories of heredity on the other. As yet, there seems little hope of any integration between the two approaches to the subject.

Criminal Youth and the Borstal System. By WILLIAM HEALY and BENEDICT S. ALPER. New York: the Commonwealth Fund (London: Humphrey Milford, Oxford University Press). 1941. Pp. vii+251. 8s. 6d. net.

Convinced that the treatment of young criminals in the U.S.A. is of a thoroughly unsatisfactory nature, the authors of this book made an investigation of the English Borstal system to determine if it was likely to prove more successful. Their investigation was unfortunately cut short by the outbreak of war, and it is not quite clear how thorough an investigation they were able to make. But they became convinced of the value of the system in reclaiming young offenders aged 16-25. After a brief outline of the American reformatory system and its shortcomings, they give a detailed description of the Borstal system, its methods, institutions and the association which is responsible for the after-care of those released on licence. Its humane nature, its care for the individual and its lack of stereotyped routine, command their admiration. The only serious criticism is of the absence of psychiatric diagnosis and the lack of psychotherapeutic treatment for the appreciable proportion of emotionally unstable and psychoneurotic individuals among the youths committed to Borstal.

The book gives a good objective account of the Borstal system. It is, however, a pity that it was not possible to carry out a longer study containing a more thorough psychological analysis of the system and its effect on the inmates of the Borstal institutions.

An Investigation of the Technique of Psycho-Analysis. Edited by EDWARD GLOVER. London: Ballière, Tindall and Cox. 1940. Pp. x+188. 10s. 6d. net.

Dr Glover, Director of Research to the Institute of Psycho-Analysis, wishes to systematize and correlate work on the technique of psycho-analysis. As a preliminary it was necessary to learn what techniques are actually employed. The results of the enquiry are published in this book.

To obtain the information a questionnaire, dealing with typical problems—methods of dealing with anxiety, problems of interpretation, transference, appointments and time-tables, fees, sex of the analyst, the relation of theory to practice, etc.—was sent to twenty-nine analysts in 1938. Twenty-four sent in replies. The results were then submitted to the Society for discussion and a supplementary questionnaire was sent out to gather information on further points. And the information so gathered is very interesting and valuable, even if questionnaire methods are suspect and though the number of 'subjects' is small. Large differences in technique are found due not only to the fact that the training of the analysts has not been uniform, but perhaps more especially because of the individual differences between the analysts themselves.

It is not possible, in a brief review, to indicate the points on which there is general agreement and on which there are differences in technique. Those who are interested must refer to the text, to which there is a very full index.

The Nursing Couple. By MERRELL P. MIDDLEMORE. London: Hamish Hamilton Medical Books. 1941. Pp. xvii+195. 7s. 6d. net.

There is little doubt that much might be done to validate or disprove the Freudian theories as to the function in the child's psychic development of the satisfaction or frustration it experiences during suckling, by making observations of the actual behaviour of infants during suckling, and relating it to subsequent development and especially to fantasy life. Dr Glover, in an Introduction to this book, ably expresses this point of view. The book itself describes clearly and interestingly observations on the behaviour during suckling of forty-six infants in the lying-in ward of a hospital; their differing modes of satisfaction and frustration; and how these were related to the behaviour and attitude of the mother. The deleterious effect of anxiety and nervousness in the mother was particularly observed. But perhaps because the author did not live to continue the work, no data as to the child's subsequent development were obtained. There are a number of observations about the effects on fantasy life such as: "If the mother bothered the child too much during unsatisfactory feeds..., he remained passive and shrinking.... I conclude that the feelings of the baby at these times would dispose him later to make fantasies of an attack during feeding...." In other words, pure speculation, even if inherently probable. The two methods of approach—observation of actual behaviour, and subsequent reconstruction through fantasy expression and free association—have still to be integrated together.

The book, however, contains a great deal of information on the treatment of babies during suckling which should be invaluable both to the mother, and also to the doctor and nurse.

Introduction to Educational Psychology. By WILLIAM CLARK TROW. London: George Allen and Unwin, Ltd. 1939. Pp. xiii+417. 8s. 6d. net.

This book begins quite interestingly with a brief but informative treatment of individual needs, emotions and personality adjustments. After that it follows the usual routine of elementary text-books on psychology, and is only very moderately interesting—although the summaries to each chapter are almost uniformly good. It

should serve its purpose as well as most elementary psychological texts. But since it is designed entirely for the American student, there seems little purpose in publishing it in this country.

The Individual in the Group. By FLORENCE M. SURFLEET. London: Allenson and Co., Ltd. Pp. 190. 6s. net.

This book consists of hints on how to run group discussions, meetings, poetry readings, etc., and includes poetry to be read. It may be helpful to some who are responsible for organizing such group activities. But if it was intended to assist people in expressing their feelings and ideas in the presence of others, the suggestions offered should have been more severely practical and concrete, and much less highfalutin'.

The book is undated.

The Collected Papers of Wilfred Trotter, F.R.S. Oxford Medical Publications. London: Humphrey Milford (Oxford University Press). 1941. Pp. v+194. 10s. 6d. net.

This book consists of the reprints of ten lectures, together with one article and a letter to *The Times*, by the late Wilfred Trotter. They deal mainly with general problems of thought and conduct connected with the study and practice of medicine; and though written with wisdom and urbanity, present no points of striking interest to the psychologist.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY, 1941

GENERAL MEETINGS

April 17-21, 1941. EXTENDED GENERAL MEETING AT NOTTINGHAM

April 17. 1st Session.

Business Session, followed by:

1. Paper: "National Character." By M. GINSBERG.
(Being Inaugural Address as Chairman of the Social Psychology Section.)
2. Motion: "That an Extraordinary General Meeting of the Society be held at 55 Russell Square on Saturday, May 31 1941, to authorize and take such action as may be necessary to secure the Incorporation of the Society."
(Proposed by the President; seconded by the General Secretary; and discussed. See Session 11.)

April 18. 2nd Session:

"The Contribution of Factor Analysis to the Study of Cognitive Ability." By J. W. JENKINS.

"A Method of Measuring After-Images." By LL. WYNN-JONES.

"The Problem of Dark Adaptation." By B. SEMEONOFF.

3rd Session (Being a meeting arranged by the Education Section).

Business of the Section, followed by papers:

"Differentiated Treatment of Reading Disabilities." By F. J. SCHONELL.

"Scholastic Ability at Different Social Levels." By C. M. FLEMING.

4th Session (Being a meeting arranged by the Social Psychology Section).

Business of the Section, followed by papers:

"International Community." By MAXWELL GARNETT.

"Field Theory in Social Psychology." By W. STEPHENSON.

April 19. 5th Session (Being a joint Meeting with the Institute for the Study of Animal Behaviour).

Annual General Meeting of the I.S.A.B.: Business of the Institute, followed by papers and films:

"Enquiry into the Place of the Study of Animal Behaviour in the Universities." Report read by F. B. KIRKMAN.

"Varied Effort in Instinctive Behaviour." By E. S. RUSSELL.
Ciné-Films: "Animal Behaviour."

April 19, 1941.

6th Session:

"The Shelter Situation as a Focus for Skilled Psychological Observation." By MARGARET LOWENFELD.

"The Mental Defective in the Army." By F. J. S. ESHER.

"Performance Tests with 'Military' Defectives." By E. J. G. BRADFORD.

7th Session:

"A Case of Hysteria—a Contrast in Theory and Practice in the Two Wars." By JOHN RICKMAN.

"The Soldier's Defences and the Public Attitude." By W. C. M. SCOTT.

8th Session.

Business of the Medical Section, followed by a paper:

"Somatic Conditioning of Air-Raid Reactions." By H. CRICHTON-MILLER.

April 20. 9th Session:

"Undeveloped Frontiers of Psycho-Analysis." By T. H. PEAR.

10th Session:

"The Nature of Shyness." By HILDE LEWINSKY.

11th Session (Being an open Session for discussion).

Among the business under discussion the Motion concerning action to secure the Society's Incorporation was put from the Chair and carried unanimously (see Session 1, above).

May 31, 1941.

EXTRAORDINARY GENERAL MEETING IN LONDON.

The following resolution was passed:

"That the Council be and it is hereby authorized and directed upon a Society with the name of The British Psychological Society being incorporated under the Companies Act 1929 as a Company limited by guarantee and not having a share capital and with a Memorandum and Articles of Association and Bye-Laws in the terms of the print submitted to the Meeting and signed for purposes of identification by the Chairman thereof (with such modifications (if any) as the Council may think fit) to give or transfer to the said incorporated Society the whole of the real and personal property belonging to the present unincorporated Society subject to the then liabilities of the said present Society and so that upon such transfer taking effect the present unincorporated Society shall be dissolved."

July 26, 1941.

GENERAL MEETING IN LONDON.

Discussion of Shelter and Evacuation Problems.

Short papers:

GROUP A. SHELTER PROBLEMS.

"A Theoretical Basis for the Study of Shelter Problems."

By P. SENFT.

338 *Proceedings of the British Psychological Society*

July 26, 1941.

"First-Aid Lectures in London Shelters: Mental Attitudes." By HERMIA MILLS.

"Children in Air-Raids of the 1914-1918 War: Their Reflections in Later Life." By G. W. GOODALL.

"Psychological Problems of Shelterers." By M. I. DUNSDON.

"Shelter Life in Birmingham." By C. BURNS.

GROUP B. EVACUATION PROBLEMS.

"War Strain in Evacuated Children." By A. T. ALCOCK.

"Children's Drawings and the War." By A. H. BOWLEY.

"The Welfare of Problem Evacuee Children." By R. H. ADAMS.

"Factors affecting Success in Billeting Evacuee Children." By R. H. THOULESS.

"Evacuation Problems and Suggested Remedies." By ETHEL DUKES.

GROUP C. MORALE.

"Researches on Effects of Air-Raids." By P. E. VERNON.

"Post-Blitz Morale." By TOM HARRISSON.

"Biological Background of Morale." By C. B. S. HODSON.

"Routine versus the Blitz." By BETTY ADDINSELL.

December 20, 1941.

GENERAL MEETING IN LONDON (Being the first Meeting of the incorporated Society).

Business, followed by:

Discussion of Problems affecting the Under-Fives in Total War.
Opening paper by C. BURT.

Short papers:

"Evacuation with Mothers." By ENID JOHN.

"Evacuation without Mothers." By RUTH THOMAS.

"Under-Fives in Bombed Areas." By GWEN CHESTERS.

SECTIONAL MEETINGS

MEDICAL SECTION

The Section has held no individual meetings during the year, but has co-operated in arranging papers given at General Meetings, notably at the 6th, 7th and 8th Sessions of the Extended General Meeting, above.

EDUCATION SECTION

April 18, 1941.

3rd Session, Extended General Meeting (see above).

A joint meeting with the Industrial Section arranged for February 6 was cancelled owing to the speaker's illness.

The Section has co-operated in arranging papers at General Meetings throughout the year.

INDUSTRIAL SECTION

- February 27, 1941. "Some Characteristic Interests of Schoolboys between 11 and 14." By W. H. N. HOTOPH.
- March 27, 1941. "Some Remarks on Incentives to Work." By MARIE JAHODA.
- May 5, 1941. "War-Time Grouses in Industry." By WINIFRED RAPHAEL.
- June 5, 1941. "The Importance of Vocational Guidance for Neurotics in War-Time." By E. L. TRIST.

SOCIAL PSYCHOLOGY SECTION

- April 17, 1941. Inaugural Address of the Chairman of the Section, given at the Extended General Meeting (see above).
- April 18, 1941. 4th Session, Extended General Meeting (see above).

A week-end Discussion Meeting of the Section, to be held in Scotland, was planned, but arrangements proved impracticable.

The Section has co-operated in arranging papers given at General Meetings, and has issued five bulletins during the year.

BRANCH MEETINGS

SCOTTISH BRANCH

- November 9, 1940. (Edinburgh.)
 "A Study of Schizoid Processes." By W. R. D. FAIRBAIRN.
 "The Personality of the Very Young Child: a Study of the Factors which Help or Hinder a Satisfactory Adjustment." By A. H. BOWLEY.
- December 7, 1940. (Glasgow.)
 "The Relative Efficiency of Different Lengths of Practice Periods in School Learning." By L. B. YOUNG.
 "An Objective Method of Marking English Compositions." By R. F. MORRISON and P. E. VERNON.
 "Fantasy and Imagination in the Perception of Pictures." By R. W. PICKFORD.
- February 1, 1941. (Edinburgh.)
 "Musical Ability and Appreciation." By H. D. WING.
 "Recognition and Déjà Vu: an Experimental Approach." By O. L. ZANGWILL.
- March 1, 1941. (Glasgow.)
 Symposium on Dark Adaptation and Night Blindness:
 "Observations on Dark Adaptation in Psychotics, using the Rowett adaptometer." By W. MAYER-GROSS.
 "Night Blindness as a Neurosis." By E. WITTKOWER and T. F. RODGER.
 "The Influence on Dark Adaptation of Music and of Temperament." By P. LAFITTE and P. E. VERNON.
 "Dark Adaptation during Stimulation with Coloured Lights." By B. SEMEONOFF.

340 *Proceedings of the British Psychological Society*

- May 24, 1941. (Carstairs.)
Symposium on War Neuroses:
Introduction: "The General Treatment of Cases of War
Neuroses." By C. D. BRUCE.
"A Survey of 100 cases." By J. D. SUTHERLAND.
"Illustrative Case Material." By H. GAUSSEN.
- November 1, 1941. (Edinburgh.)
"Compulsive Offences in Juveniles." By DENIS CARROLL.
"A Study of War Attitudes." By P. E. VERNON.
"Psychological Circumstances of Wars that Ended between
1820 and 1929 A.D." By L. F. RICHARDSON.
- December 6, 1941. (Glasgow.)
"Aspects of Musical Inspiration and Composition." By
W. GILLIES WHITTAKER.
"Aspects of Pictorial Composition and Inspiration." By W. O.
HUTCHISON.

MIDLAND AND NORTHERN BRANCHES

Neither of these Branches has found it possible to arrange individual Branch Meetings during the year, but a large number of their Members attended the Extended General Meeting at Nottingham, and several have contributed papers at this and other General Meetings of the Society. Both Branches suffer in war-time from the difficulty of arranging Meetings which can be attended by Members scattered over a wide area, but both are still in contact with local Members.